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SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

BRONCHOCELE.—*Phosphorus.*—If the usual iodine treatment proves of no service, try the internal administration of phosphorus in doses of from one-fiftieth to one-twentieth of a grain. (Mr. S. M. Bradley, Brit. Med. Jour., Nov. 29, p. 630.)

FEVER.—*Cold Baths.*—The use of cold baths in fever is of great value when the temperature is very high. The cold, however, does not restrain the excessive heat, it only relieves from its consequences. Its action is thus diametrically opposed to that of alcohol, quinine, and other agents, which directly moderate the febrile combustion; its *rôle* is that of spoliation, not of economy of waste. In slow, chronic fever, there is no danger from the elevation of temperature, but in active pyrexias the danger is great, and our duty before all things is to cool the blood by the rapid extraction of heat from it; an indication which hydrotherapy alone is capable of fulfilling. By means of a few baths administered in each twenty-four hours, we succeed in beating down and mastering a febrile movement which everything seemed to show would be violent and lasting. In satisfying this essential indication, the lowering of temperature, hydrotherapy weakens at the same stroke almost the whole group of the symptoms of typhoid pyrexia. (Prof. Behier, p. 15.)

Alcohol as a Reducer of Temperature in Fevers.—Abundant evidence goes to prove that alcohol possesses the power in acute pyrexial diseases of fulfilling those two objects so desirable in the treatment, viz., reducing the temperature and checking the tissue waste of the body. Hence, the "empirical facts" of Todd are borne out by the results of scientific investigation. In the early days of typhus fever the urea excretion is *double*, implying great waste of the muscular tissues, including the heart. How important in this disease, then, must be the early use of a remedy which possesses the power of checking this morbific process! (Dr. Anstie, Practitioner, Dec., p. 422.)

Quinine as an Antipyretic.—Quinine has a certain and marked antipyretic power, but it is necessary to use it in suitable stages of proper cases. It is no use giving heavy doses of quinine to depress the elevation of the first four days of typhus or pneumonia, or in the hyperpyrexia of rheumatic fever, in all which it would probably be positively harmful. When we have to meet immediate danger from a rapid rise, I put no trust in quinine, but resort at once to cold baths, bags of ice, and the like. If, however, a high temperature is doing harm at the latter part of a period, quinine will often act like a charm. If, for example, towards the end of the third week of typhoid, a temperature of 104°·5 or 105° is likely to be too much for the frame already too far consumed to resist it as it would once have done, then 20 grains of quinine is often very valuable; and such doses, repeated as may be required, may determine the close of the period and release the sufferer. In the second week we get no such success as this. (Dr. T. C. Allbutt, p. 12.)

LUMBAGO.—If internal remedies, such as iodide of potassium and opium have had no appreciable effect, apply a continuous galvanic current, at the same time making the patient exercise the affected muscles rhythmically, by bending, extending and rotating the spine. From 10 to 18 cells of Mayer and Meltzer's zinc carbon battery should be employed. The positive pole is to be placed at the upper part of the spinal column, in the middle line, and the whole back including the intercostal muscles, is to be sponged with the negative pole. (Dr. G. V. Pooer, Lancet, Dec. 27, p. 899.)

RHEUMATIC FEVER.—*Quinine.*—Excess of acid is one of the never-failing accompaniments of rheumatism. In the treatment of this disease usually, we pour large and frequent doses of alkali into the blood, in the hope that it will be distributed to every part of the body; but we find that, in spite of all our efforts, with the exception of the kidney secretion through which the excess of alkali is being constantly expelled, the acid continues to show itself wherever we can search for it; the other secretions continuing acid in spite of the excess of alkali in the blood. The endeavour to neutralise the acid is clearly rational, but it may be questioned how far we succeed in attaining our object. In fact, whatever may be said of the alkaline treatment of rheumatic fever, it cannot be called curative in the proper sense of the term. It may help to shorten the duration of the attack, it may serve to lessen the intensity of the inflammation, it may diminish pain, and it may not unfrequently ward off those more serious consequences which give to rheumatic fever its formidable

character as a cause of heart-disease. All this it may do, and in my own opinion, and that of many others who have made the experiment, it does actually accomplish, but it does not cure. It does not put an end to the paroxysm, as bark puts an end to an attack of ague; it does not eradicate the disease, as iodide of potash will eradicate the traces of syphilis, commencing as soon as the system is sufficiently saturated by full doses of the remedy. Many cases of acute rheumatism treated with alkalies give indications of depression, which can only be explained as the effect of the long-continued use of the remedy. . Various means have been tried to meet cases of this kind. Stimulants avail to a certain extent, but have seemed to me to tend of themselves to keep up the disease, to lengthen its duration, and even to re-excite it after it has subsided. Quinine, is that which, on the whole, has answered best in my experience. There is frequently in these cases phosphatic deposit, and this deposit rapidly disappears under the use of quinine. (Dr. A. W. Barclay, p. 44.)

Acids.—We must all feel dissatisfaction with the ordinary methods of treating acute rheumatism. There is no proof whatever, that the alkaline method is of any value, and much that mint water produces quite as good results. In one case 15 drops of dilute nitro-hydrochloric acid was given every four hours, with the effect of relieving the joint affection, almost completely in nine days,—curiously enough the urine being alkaline on recovery, although no alkali had been administered. Dr. Wilks maintains that, until we know everything concerning the disease, and especially the relation between the joint affection and the internal complications, we have no scientific basis for the selection of remedies. It is even doubtful whether the subsidence of the arthritic inflammation during the course of the disease is desirable, for in all fatal forms of rheumatism this is usually seen to occur. It may be remarked, however, that sometimes, in cases of internal complication, it does seem as if large doses of potash or of quinine have some influence in arresting the disease. In several cases treated by quinine Dr. Wilks has noticed the urine to become alkaline on recovery, as if this were the natural process of cure; as is seen also in typhoid fever. (Dr. S. Wilks, p. 51.)

AFFECTIONS OF THE NERVOUS SYSTEM.

FEIGNED HEMIPLEGIA.—*Detection of.*—In these days of railway accidents it is necessary that we should have clear ideas of the means of diagnosis between real and feigned diseases.

Where treatment by powerful shocks of electricity is available, it will usually be easy to cause such pain as will lead to the exposure of feigned anæsthesia. The symptoms of motor paralysis must be carefully investigated; and if the paralysis is complete, or nearly so, it will not be difficult to distinguish between the real and the feigned disease. Several tests are available. 1. The well-known appearance of dragging in a paralysed leg. This symptom was at first entirely absent in the case related, the slight dragging which was subsequently observed, differed from that of ordinary paralysis. 2. The test suggested by Dr. Hughlings Jackson. In real hemiplegia the paralysed arm falls forward when the patient stoops; in case of feigning, the arm will probably be retained by the side. 3. The condition of the muscles in long-continued paralysis. Where paralysis has continued for several months in a limb, we should expect that the muscles would both diminish in size and lose their electro-contractility. 4. In cases where the patient says that he has been confined to bed for many weeks, the condition of the soles of his feet may be taken as a test of the correctness of this statement. A scaly and cracked condition of the cuticle of the soles will, according to our experience, be invariably found in adults (at least in the lower ranks of life) who have been confined to bed for a considerable time. Where anæsthesia is feigned, it may be detected by the unexpected application of a needle—the patient's attention being at the time taken up by the examination of another part of the body, such as the eyes. (Dr. A. Davidson, p. 65.)

MALINGERING.—Where the physician's suspicions are aroused, faradisation of the skin with a wire brush and a powerful current, may be an excellent means of settling such questions. This proceeding is extremely painful, and yet never does any harm, except in cases of cerebral disease, where it should be avoided. We need, therefore, not be reluctant to employ it in suspicious cases, as it is far more humane than the actual cautery which has often been called into requisition, and yet sufficiently disagreeable to make a repetition of the procedure dreaded by such persons. As in peripheral paralysis the muscles always lose their faradic excitability, the presence of the faradic response in the muscles of the arm enabled us in one case to state decisively that the man was shamming. (Editor of Brit. Med. Journal, p. 70.)

NERVOUS MIMICRY OF DISEASES OF THE SPINE.—It is very necessary to diagnose nervous mimicry of diseases of the spine from true spinal disease. If there be no considerable attendant illness, an intense and horrible pain in or at the

spine does not mean serious disease of the spine. Pain of this kind may be due to aneurism or cancer, or to some distant malady in a nervous constitution, but it is not a sign of primary disease of the spine. Excessive tenderness is also characteristic of purely nervous disorder. If the patient flinches or writhes when the finger taps or presses very gently, we may be sure it is not real spinal disease. Another usual character of this pain is its frequently extreme increase in fatigue, not only in bodily fatigue, as in long sitting upright or standing, but in long occupation of any kind, even in mental occupation if it is not very agreeable. With this character, also, it is common for the pain to be accompanied by nausea or even vomiting, and it is a nearly sure sign of merely nervous disorder if pressure on the spine produces shivering or nausea, or a feeling of going to be sick. And, lastly, it is a sign of nervous pain alone if the pain has lasted many weeks or months, and nothing has come of it; no immobility of the spine or ribs, no paralysis, no fever, or wasting, or great disturbance of the general health. When we find a patient carefully—not with muscular spasm, but with care—holding his head and neck, or any part of his back very still, turning himself, or bending cautiously, we must look for disease of the spine. The opposite state of this in which the patient tumbles this way or that, like a baby, is sure to be without organic disease. (Sir J. Paget, p. 53.)

NEURALGIA. — *Phosphorus.*—After trying several preparations, I now use a formula which Mr. Potts, dispenser to the Manchester Royal Infirmary, hit upon, and which seems to answer every purpose, in being tasteless, transparent, and readily prepared. He dissolves ten grains of phosphorus in two ounces of ether, agitating the solution from time to time; and of this solution, one minim (containing one-hundredth of a grain) is administered in an ounce of water with half a drachm of glycerine. The glycerine suspends the phosphorus so perfectly that a transparent mixture is the result. The addition of a little bitter infusion entirely removes any *soupcon* of lucifer-matches which may hover about the medicine. (Mr. S. M. Bradley, p. 71.)

Supraorbital Neuralgia.—Subcutaneous Injection of Strychnia.—A case of supraorbital neuralgia, which had not been relieved by the administration of quinine, was cured by the subcutaneous injection of strychnia. The solution used contained four grains to the ounce, and of this two minims were injected at a time. The first injection gave relief, but the pain did not completely disappear till a third had been given. The

injections were given at intervals of two days. (R. A. J., p. 71.)

PHANTOM TUMOURS OF THE ABDOMEN.—There is often some little difficulty in diagnosing phantom tumours when they occur in the abdominal muscles. They are sometimes large, hard, and nearly fixed in place, size, and form; and they may be deceptively complicated with disorderly states of the intestines, or the aorta, or other abdominal or pelvic organs. But in any case, however difficult the diagnosis, the use of ether or chloroform will decide the matter at once. With complete insensibility all signs of tumour vanish. (Sir J. Paget, p. 59.)

TETANUS.—*Injection of Chloral.*—In a case of traumatic tetanus in which the mouth became so closed that no remedy could be administered, M. Oré of Bordeaux threw an injection containing ten grammes of chloral into the veins, with the effect of producing peaceful sleep; and this was followed by a second and third injection, with the effect of obtaining a sleep of eight hours. (M. Oré, p. 72.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

ANEURISMS.—*Galvano-puncture.*—It is only sacculated aneurisms of the arch of the aorta or of other great arteries of the body which can be treated by galvano-puncture with any prospect of success. We may get all the good and none of the bad effects from Fobeaux's modification of Smee's battery, made by Weiss and Co. It is upon the question as to the most advantageous mode of employing the current that the greatest differences of opinion exist. Some writers recommend the insertion of the negative pole only into the sac, whilst the positive pole is applied to the integuments outside. Others recommend the introduction of two needles into the sac, one of which is to be connected with the positive pole and one with the negative, the current being allowed to pass all the time in the same direction, and in the hands of different operators, being either strong for a short time, weak for a long time, or even strong for a long time. But some of those who recommend the insertion of both poles into the sac, follow Pétrequin in advising that the direction of the current should be changed from time to time, during the same operation. And, lastly, a third set of writers recommend the introduction of a needle or needles in connection with the positive pole into the sac itself, whilst the negative pole is applied by means of a wet sponge to the

adjacent skin. The introduction of the positive pole alone, with the application of the negative pole to the neighbouring parts, always brought about a rapid and complete coagulation. It would seem, therefore, that this mode of procedure is the best, even from the point of view of the readiness with which coagulation is brought about, and its satisfactory nature. The needles previously employed by most operators seem to have been larger than it is at all necessary to use, whilst some of them, such as Weiss's modification of Dr. Duncan's needle, are unquestionably very much too large. Recognising the desirability of reducing to a minimum the chance of hemorrhage through the punctures and of inflammation in the track of the needle, and, at the same time, of diminishing the painfulness of the operation, I was led to use a very small hare-lip needle. The experiments of Dr. Fraser and others have unmistakably established the fact that the coagulation induced in the blood by the passage through it of a galvanic current, is wholly attributable to the electrolytic or chemical changes which the current occasions. If, therefore, after the fashion originally introduced by Steinlein, we cover the positive pole with an easily oxidisable metal, such as zinc, we are enabled to summon to our aid what Faraday always distinguished as the 'secondary' effects of electrolysis. The salts as well as the fluid of the blood are decomposed by the action of the current, and a part of the oxygen and acids which are liberated at the positive pole will combine with the zinc and corrode the surface of the needle. Some of the salts thus formed will aid in producing further coagulation around the needle. With regard to the strength of the current, it was found that eleven cells acting for half an hour, although producing no pain or obvious sensation at the time, did induce some amount of irritation of the skin around the punctures. The current from eight cells, however (the battery being in good condition at the time), produced, on the previous occasion, no irritation, so that all traces of this needle-puncture had disappeared in four or five days. This needle also was insulated within half an inch of its point, merely by two layers of spirit varnish; and probably, in employing a comparatively weak current such as this, no insulating material is needed. This, indeed, is the opinion of Dr. Althaus. At all events, it seems desirable that the needle should not be pushed too far into the sac, and that the part of the needle actually in the sac and within its coats should not be insulated, because clot should form in contact with the wall of the aneurism, so that it may be left adhering to it on the withdrawal of the needle. (Dr. H. Charlton Bastian, p. 190.)

Aneurism of the External Iliac Artery.—Lister's Abdominal Tourniquet.—Aneurism of the external iliac artery may be cured by pressure upon the abdominal aorta just above its bifurcation. Chloroform should first be administered, and ether substituted for it as soon as the muscles are relaxed. In a case related by Mr. Wheelhouse the anæsthesia was kept up continuously for five hours, without any untoward symptoms, twenty-five ounces of ether being used. "Lister's large abdominal tourniquet was applied just over the umbilicus, and slowly screwed down until the flow of blood through the aneurism was arrested. Two slips of the instrument occurred during the first half hour, but it was at once replaced. Pressure was commenced at 1.15 p.m. The foot was cold and a little blue at 2.30 p.m.; at 3 p.m. the tourniquet was unscrewed slightly; pulsation in the tumour returned, and it was at once reapplied. At 4 p.m. blueness had extended beyond the knee, the left leg being very cold; at 5 p.m. the right limb was blue to the groin, and the left to the knee. Pressure was then slightly relaxed; the tumour much harder, but pulsation still perceptible. At 6 p.m. both limbs were black, and body blue as far as tourniquet. The instrument was then removed gradually in fifteen minutes, a quarter turn of the handle being taken every minute. The tumour had ceased to pulsate, and was firm and hard." (Mr. C. G. Wheelhouse, p. 188.)

ARTERY AND TORSION FORCEPS.—*Mr. Spencer Wells's New.*—These forceps have scissors handles. The grasping and holding extremity is roughened by rather deeply cut transverse teeth, so that the bleeding vessel is forcibly compressed, and its coats squeezed or almost crushed together. This is alone often sufficient to stop the bleeding without any torsion, especially if the instrument be left on the vessel for a minute or more. But if the vessel be large, then two or more rotations may be added. Instead of the spring catch, the fastening is effected by a Mathieu's catch in the handles. This is quite as easily fixed and opened as the spring, and is much less likely to get out of order. The instrument is made of steel, but is coated with nickel, which prevents any rusting after use. (Mr. T. S. Wells, p. 202.)

ERECTILE TUMOURS.—*Croton Oil.*—It is sometimes difficult to eradicate small erectile tumours. The following plan is very successful and easy of application: Take a piece of cork and pass through it a number of fine needles, so that the points protrude for about two millimètres only. These points should be disposed so as to represent the shape and size of the spot to be operated on. The cork is now to be dipped in croton oil,

and then accurately applied over the spot and quickly pressed upon it. A little cotton wool is to be used for dressing. The next day there is a little swelling and vesication. A second application at the interval of a few days, will, if as successful as the case from which this is taken, cure the nævus without leaving any scar. (Dr. De Smet, p. 212.)

EPISTAXIS.—*Improved Plug.*—In severe cases of epistaxis the nose may be plugged by means of an elastic india-rubber bag inflated after introduction. For this purpose, I cut a No. 4 gum-elastic catheter into two equal lengths. At the end of one portion, I made small holes by transfixing it with a needle heated to redness in a spirit-lamp. These holes extended, at intervals of a quarter of an inch, for the length of about two and a half inches. Over this punctured portion I tied firmly in two places, first at the tip, and then again about three and a half inches nearer the handle, a soft India-rubber bag, so as to include between the two tied portions all the perforated part of the catheter. To the open end of this catheter I fixed a short piece of India-rubber tubing furnished with a small stopcock. All the air having been let out of the bag by opening the stopcock and gently compressing the India-rubber, somewhat after the manner of fastening an umbrella, the instrument is passed into the bleeding nostril. When it is coaxed so far that the end projects into the posterior nares, or as far as it can be, the nose of a small ball-syringe is applied to the mouth of the stopcock, and the bag is gently inflated while the air is being propelled into it with the right hand. The short piece of India-rubber tubing attached to the stopcock is compressed with the thumb and finger of the left hand at the right moment to prevent the air from passing out again; and then with the right hand the stopcock is turned. By this means, the inflated bag is made to fill all the inequalities of the nasal cavity, and a painless and perfect plug results. (Dr. H. C. Rose, p. 208.)

MIMICRY OF ABDOMINAL AND OTHER ANEURISMS.—In hysterical females there sometimes occurs an abnormal nervous condition of the abdominal aorta, which closely simulates aneurism. The same thing may occur in the subclavian, innominate, and carotid arteries, but more rarely. The chief characteristic of the mimicry is that the affected artery feels much larger than it should be, and pulsates largely at least in one direction, dilating obtrusively, and often so much more visibly than in an ordinary person that it is hard not to believe that it is largely dilated. And yet there certainly is no considerable dilatation; it is rather as if the arterial walls were thin and had too little muscular resistance, as we might suppose them

to be in a condition of partial paralysis of their muscular tissue. In the most marked case of imitation of abdominal aneurism that I have seen, examination after death found no disease. (Sir J. Paget, p. 60.)

NÆVUS.—*Sudden Death from Injection of Perchloride of Iron.*—Mr. Kesteven relates a fatal case from injection of five minims of perchloride of iron into a nævus. The child suddenly turned pale and was slightly convulsed, at the same time that it began a series of eight or nine shrill laryngismal cries, attended with distinct struggles to recover its breath, which suddenly ceased in death. In the Lancet for August 17th, 1867, Mr. Thomas Smith of St. Bartholomew's, has collected several fatal instances following shortly upon the injection of nævi with perchloride of iron. In these cases the nævi were situated upon some part of the face or near the veins in the neck. As the result of these fatal consequences, Mr. Smith concludes:—"Sufficient is known of the effect of the possible admixture of perchloride of iron with the general circulation, from injecting nævi on the face, to justify us in rejecting it as a remedy for nævi in these parts, unless, by pressure or by the employment of some instrument, the circulation in the growth is controlled, at least for some time." (Mr. W. B. Kesteven, p. 212.)

Fatal Results of Treatment by Injection of Perchloride of Iron.—However tempting it may appear to treat nævus in a prominent situation on the face by injection of perchloride of iron, because of the absence of scar, this mode of operating must on no account be adopted. Mr. West, of Birmingham, relates a case in which death resulted from embolism, a clot was found stretching from the right internal carotid artery along the middle cerebral artery of the same side. (Mr. J. T. West, Lancet, Mar. 21, p. 402.)

TORSION OF ARTERIES.—The principles of torsion are based on a knowledge of the anatomy and physiology of arteries. The arteries contain in their walls a muscular tissue, which contracts and retracts whenever the vessel is divided, the more so if it is rudely lacerated. An objection to torsion is to be found in the fact that the arteries are liable to be extensively diseased, so that it no longer fulfils its natural functions. Torsion is, moreover, not adapted for cases where the artery cannot be sufficiently withdrawn to allow of free twisting, and is useless where hemorrhage occurs from the side of one vessel, as when a branch has been divided close to the trunk. It appears, therefore, that torsion requires for its complete and safe employment a healthy subject, sound arteries, good instruments, and a

persevering operator. The principles of torsion are, that by twisting the vessel the muscular coat is ruptured and becomes contracted, and that its roughened edges act as foreign bodies, which whip out the fibrin, and on which lymph is subsequently deposited, it seems practically to be a matter of small importance whether the end of the vessel is twisted off, or whether only a few turns are made. But the question in dispute is whether the bruised and twisted end sloughs off or retains its vitality. The truth is, that much depends on the size of the vessel and the manner in which torsion is performed, for if the piece have not been severely bruised it may preserve its vitality; but if it have been twisted too frequently, or if the tissue be much broken, there can be no doubt that the end does often slough off and act as a foreign body. (Editor of Lancet, p. 125.)

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PLEURISY.—There are some cases of very limited pleurisy, having but little tendency to spread, which may be checked by mechanically fixing the entire side, so as to restrain the movements as much as possible. The best plan is to use strips of plaster, putting on two or three layers in the following manner:—The first strip is laid on obliquely *in the direction of the ribs*, the second *across the course of the ribs*, the third in the direction of the first, about half overlapping it, the fourth as the second, and so on until the entire side is covered. A strip is also passed over the shoulder, which is kept down by another fixed round the side across its ends. (Dr. F. T. Roberts, p. 72.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

DYSPEPSIA.—*Papya Juice a Solvent of Nitrogenous Food.*—The papya tree is indigenous to tropical climates—it is common in Bengal. When the unripe fruit is scarified, it yields an abundant flow of a milky juice. This juice when added in very small quantities, such as a few drops, to any nitrogenous articles of food, immersed in a little water, has a wonderful solvent action upon them. It is extensively used for this purpose amongst the native cooks in India. A few drops of the fresh juice were added to a pound of minced goat's meat, which was stewed over a slow fire. The whole ran into a diffluent mass in five minutes, owing to a larger quantity of the juice having been used than was necessary to make the meat tender and eatable. What the peculiar nature of the

action is, is uncertain; it is not that of a caustic, for the juice has no taste of consequence. The whole action is so identical to healthy digestion that I wonder we have not availed ourselves of this medicinal property, in cases of invalids and dyspeptics, to substitute a process of artificial digestion. A few grains taken immediately after a meal will substitute the power where it is wanting. For medicinal use the fresh juice should be dried in the sun in shallow dishes. (Dr. G. C. Roy, p. 75.)

CLEFT IN THE HARD PALATE.—*New Operation.*—Sir W. Fergusson has successfully adopted a new plan of operating. The first steps of this operation are somewhat similar to the old operation for closing the cleft in the hard palate—namely, paring the edges of the cleft, and making an incision down to the bone parallel to, and about a quarter of an inch from, the edge of the cleft on either side, the point of the knife being carried back just as far as the junction between the hard and soft palate. Into these incisions a chisel half an inch broad is inserted, and its edge directed against the posterior margin of the hard palate and made to cut from behind forwards, thus partly detaching a slice of bone on each side, with the soft tissues and periosteum attached to their upper and lower surfaces. The result of this is that the sides of the cleft fall easily together, leaving a small aperture through the bone on either side. One, two, or, if the fissure be long, three stitches are passed through the lateral clefts by means of an ordinary aneurism-needle, and thus encircle the detached portions of bone and soft tissue, each suture passing through into the nasal cavity. From the liability of the flaps to twist in slightly, and from the thinness of the edge, Sir William Fergusson is careful to pare the sides somewhat obliquely, in order to present wider raw surface for adhesion. The sutures, which are kept in much longer than in the ordinary operation, cause no harmful irritation. The lateral clefts become filled up by new bone, which is rapidly thrown out and tends to keep the parts firmly united in the median line. (Sir W. Fergusson, p. 217.)

EPITHELIOMA OF THE LIP.—*The Elastic Ligature.*—The author relates a case in which the lip was pierced at a spot below, but quite clear of, the disease, and two india-rubber cords passed, by means of which the whole mass of disease was included. Fourteen days were required for the complete separation of the included part. Powdered charcoal had to be kept upon the part on account of the smell. (Dr. H. S. Gale, p. 153.)

FISTULA-IN-ANO.—*The Elastic Ligature.*—There are some cases in which from the patient's habits and constitution hemorrhage may be a matter of serious consequence. If, therefore, in these persons the fistula is one of considerable length, the elastic ligature is preferable to the knife. The ligature is introduced double by means of a probe, which is then again withdrawn, leaving the loop high up within the rectum. The loop is then drawn out, and the double ligature having been put tightly on the stretch is securely fastened. A morphine suppository will effectually relieve the pain produced. Although I entertain grave doubts as to the alleged immunity from the occurrence of pyæmia, which Professor Dittel claims for this method, I am confident it will be in many instances preferred by operating surgeons to cutting instruments, when, as Professor Dittel observes, "he has to undertake operations in cavities and canals which are out of sight, or which are so narrow that it is extremely difficult, if not impossible to use cutting instruments within them, or when he feels uncertain whether he may be able to tie the bleeding vessels (as in anal fistulæ situated high up). In other cases he will desire to avoid hemorrhage and the formation of large wounds in children or in old persons." (Prof. W. Stokes, p. 257.)

HARE-LIP.—It is much better to operate for the cure of hare-lip soon after birth than to delay the operation to a later period of life. The best time is three weeks to three months after birth. In the case of double hare-lip, with double cleft in the alveolar ridge, there may be great projection, or little or none. In the latter case, particularly if the columna and lateral portions of the lip be of good size, there may be no need for meddling with the intermaxillary mass. If, however, the projection be considerable, or what may be called great, and if the columna and side portion of lip be scanty, there ought then, in my opinion, to be no hesitation about taking away the projection at its junction with the vomer. The attempt to push this part back by gradual pressure is troublesome, or well-nigh impossible, in most instances, even if, as has been proposed, its narrow neck be broken. I never hesitate to remove the intermaxillary mass when it seems the least in the way of a satisfactory operation. The instances where there is no special projection are common, and require no comment, as there is then, as regards this matter, no obstacle to a satisfactory and successful operation; but, when there is a projection, if considerable, it is a more serious obstacle to these results than those inexperienced may imagine. I believe that this condition is a frequent cause of

failure in the ordinary operation, particularly if it be done without the truss-compressor on each cheek to push the lateral portions of the lip towards the mesial line. The projecting portion should be removed as high up towards the nostril as possible, and here in infants, as there is only cartilage to divide a pair of scissors is all that is necessary. Whilst I can offer little objection to this proceeding, I fancy that I have recently fallen on one equally efficacious and void of certain objections which, I think, might be urged against it. Instead of this sweeping wholesale abstraction, I content myself with making an incision, vertical, sloping, or horizontal, with a scalpel through the mucous membrane and periosteum, over the projecting piece of bone; with a few touches of the knife, or a little squeeze with finger and thumb, I so separate these tissues as to permit the entrance of a gouge of a quarter or three-eighths of an inch in breadth, with which I scoop out the body of the milk incisor-tooth in as far as it is formed, taking no heed of the cyst or of that of the permanent one, and even cut out such wall of bone as may be there; usually, at four or eight weeks, only small plates of bone. In this way, the hard projection is removed, and the tissues that remain offer no obstruction to the union of the junction of the lip in front, whilst the operation, as it appears to me, is less destructive, therefore more conservative, in character. There is thus left only the mucous membrane, with possibly some periosteum, which form a soft cushion behind the wound in the lip, and so the remaining intermaxillary bone is not divested of covering so thoroughly as when cutting instruments are passed in the mesial line to take all away on the offending side. (Sir Wm. Fergusson, p. 214.)

HEMORRHOIDS.—*The Elastic Ligature.*—This mode of operating is especially adapted for hemorrhoidal tumours, nævi, and warts, and I should have used it more frequently than I have done had not experience proved the superiority of the curved clamp and the actual cautery where there is no objection to their use. (Mr. Hy. Lee, Med. Times, Dec. 6, p. 648.)

Dr. Richardson's new "tooth-edged cutting scissors" are an admirable instrument for snipping a groove at the lower edge of internal hemorrhoids prior to ligaturing them, as no bleeding occurs and the view of the part is consequently not obscured. (Mr. C. F. Maunder, p. 365).

STOMACH PUMP.—*Dr. Tosswill's Improved Syphon.*—The ordinary stomach pump is both costly and cumbrous; moreover, its use is not unattended with danger, cases having been known where the tube has been pushed through the coats of

the stomach. A much simpler and better instrument is one which has been recently made for me by Messrs. Arnold of West Smithfield. "The advantages I claim for my instrument are—freedom from danger, cheapness, and increased portability. It can be easily carried in the coat-pocket, its cost does not exceed 15s., and it is almost impossible to perforate the coats of the stomach or œsophagus by means of it, be the operator ever so clumsy or ignorant. The instrument consists of an indiarubber tube, about half an inch in diameter and some four feet in length, provided with a ball of the same material, which is three or four inches in diameter, and situated about eighteen inches from one end of it. In fact, it is almost a *fac-simile* of one of Higginson's enema syringes, but without a valve." The tube is nothing more nor less than an enlarged Thompson's indiarubber catheter, with two large oval holes situated near the extremity. (Dr. Louis H. Tosswill, p. 85.)

THREAD-WORM.—Santonine, although almost a genuine specific for round-worm, is of little or no use in the treatment of thread-worm. Injections of quassia, lime water, salt, and castor oil, although affording partial relief, avail nothing towards a permanent cure. It is a mistake to suppose that the thread-worm inhabits only the rectum and sigmoid flexure of the colon. The fact is, that the entire length of the colon is the territory inhabited by the thread-worm. The cæcum itself constituting the parasites' true head-quarters. It will be seen at once from this that we must rely on medicines given by the mouth, rather than on injections. Speaking generally, I give a preference to active saline cathartics repeated for several days in succession, followed by the use of cold water enemata. Small doses of chloric ether and sulphate of iron are eminently serviceable additions, and the same may be said of aloes, with or without assafœtida. In cases where these drugs are objected to, I have employed various active mineral waters with good results. Bitters of all kinds are useful, and patients who object to salines will swallow any reasonable amount of the infusion of gentian and other pure vegetable tonics. The rapid passage of the drugs through the bowel, will, if several times repeated, carry most of the parasites sufficiently low down to be within reach of the clysters; and the prevention of the return of the parasites to the upper part of the colon is one of the practical points worth looking to. The employment, therefore, of enemata, after the exhibition of remedies by the mouth, cannot fail to prove beneficial. (Dr. T. Spencer Cobbold, p. 87.)

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AFFECTIONS OF THE DIGESTIVE SYSTEM.

DYSPEPSIA.—*Papya Juice a Solvent of Nitrogenous Food.*—The papya tree is indigenous to tropical climates—it is common in Bengal. When the unripe fruit is scarified, it yields an abundant flow of a milky juice. This juice when added in very small quantities, such as a few drops, to any nitrogenous articles of food, immersed in a little water, has a wonderful solvent action upon them. It is extensively used for this purpose amongst the native cooks in India. A few drops of the fresh juice were added to a pound of minced goat's meat, which was stewed over a slow fire. The whole ran into a diffluent mass in five minutes, owing to a larger quantity of the juice having been used than was necessary to make the meat tender and eatable. What the peculiar nature of the

LITHOTRITY.—*The Future of.*—I have come to the conclusion that stone in the bladder, like many other maladies, is an exterminable one. I believe that this grave malady, which has tested surgical skill during two thousand years and produced a ponderous literature, which has been dreaded beyond all things by mankind, and has been the source of untold suffering to thousands in every age, is nevertheless a malady which may be exterminated; so far, that is, as it is a painful and dangerous one. The sources of danger in lithotrity are but two in number: injury to the soft parts by the instruments employed; and injury to those same parts from the sharp edges and angular forms of the fragments which are produced by the process. When we have reduced the mechanical action to the minimum of capability to inflict mischief, and have learned to make fragments in such a manner that they shall produce the least amount of irritation, why then we shall have arrived at perfection in lithotrity. With a calculus of certain weight and dimensions, whether of uric acid, phosphates, or oxalate of lime, say not exceeding the volume of an ordinary nut, a perfect result may be ensured. With the size of the stone, however, the danger increases. With stones of middle size a certain small proportion of deaths is met with, perhaps one in twelve or thirteen cases, and with stones of larger size the mortality is one in eight or ten cases. Then there must always be a number of cases in which the choice of operation hangs on the slightest circumstance. There is a wide border-ground, so to speak, which is common to both operations. No man, whatever be his experience can lay down a hard and fast line, and say, "On this side lie the circumstances which determine lithotrity, and on the other side are the circumstances which make lithotomy imperative." The border-ground is much broader than I often like, and there are a good proportion of cases in which it is impossible to predict which operation of the two will be most successful: stones which are just large enough to be crushed, which are certainly not outside of the limits of size for lithotrity, and in patients of a certain class for whom a half-dozen sittings will not tell heavily. On the other hand, the crushing of the same stone in the sensitive patient with a worn-out nervous system may make too large a demand on his endurance. Thus you see the only certainty of attaining success lies in getting the stone to operate on when it is small. And success being absolute then, as I think I have been able to prove beyond all manner of doubt, it follows as a matter of necessity—That the Diagnosis of the presence of stone in the bladder and of its

size is a matter of the highest importance. Next to the best operation for the removal of large stones from the bladder, the most important question is the best method of discovering the existence of very small stones in the bladder. First, respecting the mode of sounding. It is absolutely essential to employ a light sound, which can be easily turned in the bladder and urethra. Nothing but a quick and delicate movement will elicit an audible note, or produce a sense of contact, with so small a body as a pea lying in the interior of the bladder. Therefore it is better to have an instrument which will roll easily between the finger and thumb, and not require the wrist or the arm to create the movement. Hence the handle should be cylindrical—like the handle, but smaller, which I originally designed for my lithotrite, and which is now much used here, and is almost universally employed abroad. The beak should be very short, so as to be turned with the utmost facility. To find a small stone the bladder should be empty or nearly so. I prefer a patient to make water a few minutes before sounding, and certainly never to be injected or prepared in any way, which only tends to defeat our object. Let him lie down with his pelvis a little raised, and then let the instrument gently glide down the urethra; it is five to one, however small the stone may be, that it is just grazed as the beak passes through the neck of the cavity into the bladder. This is perceived easily if the sound is only guided lightly by the faintest touch of the finger and thumb. Held and guided by the wrist and arm so slight a graze may be unnoticed. If not felt, let the sound make two or three quick semi-rotations right and left: if still nothing found, depress the handle slightly to turn the beak below, close to the neck of the bladder, and make two or three similar movements there. That is where the stone will be found, if there is one, in the nearly empty bladder; and in the same manner a small fragment will be found at the close of a case. I assert that more than half the stones I operate upon are found in cases in which no suspicion as to the real malady has arisen until the sound has been employed. Now, with the utmost deference to others, and only after the acquirement of a profound conviction on my part, I venture to say that it is my belief that the early signs of calculus are not generally sufficiently recognised." (Sir Henry Thompson, p. 224.)

STONE IN THE BLADDER.—*New Instruments for the Detection and Extraction of.*—Mr. William D. Napier describes some new instruments which we think will prove exceedingly

useful. The Calculus Detector is a sound, the terminal two inches of which is coated with pure lead. Before introduction this is to be highly polished by friction with leather, and to be examined with a lens both before and after introduction into the bladder. The lead is so impressible that it is certain to be marked by contact with a rough substance like a calculus. The Calculus Extractor is for the removal of small stones and fragments of stone. "It consists of a soft India-rubber tube, terminating at one extremity in a funnel-shaped orifice, greatly resembling the ordinary convolvulus flower, and it is capable of containing a calculus or portion of calculus of considerable dimensions. When it is inserted into the bladder, its form and position render it the receptacle of such foreign substances as would from the position of the patient and the flow of the urine determine towards the natural outlet. This receptacle, enclosing such bodies in the folds of its delicate membrane, and permitting itself to be gently withdrawn, retains them in its elastic envelope, and protects the urethra from direct contact with them." The instrument is introduced into the bladder thus:—Having been soaped, it is inserted into a silver catheter open at the end, the whole, including the convolvulus-shaped cup, being withdrawn within the tube. The end is closed by a piece of cocoa butter, which lubricates the passage as the instrument is inserted, and rapidly melts in the bladder, so as to allow of the extrusion of the India-rubber cup. The Extractor may also be made use of as a self-retaining catheter. (Mr. W. D. Napier, p. 232.)

Stricture of the Urethra.—The condition of the parts in stricture of the urethra is hardly ever a purely physical one. With the structural change there is associated an active morbid state, either some inflammation or congestion of the parts, and almost always more or less of spasm. How else can we account for the varying size of the stream, even when permanently diminished. It follows from this that the plan of treating stricture by purely physical means is founded on a misconception. The patient should be kept in bed in order to secure complete rest in the horizontal position and uniform warmth. Opium has great power in allaying irritability; it is best administered as laudanum by the rectum. Active aperients are often strikingly serviceable. The use of instruments must be kept in the background until all morbid action has passed by, and they must not even then be used unless we are driven to it, for it is no uncommon thing for effused lymph to be absorbed under favourable circumstances. While I am not suggesting that

instruments are never to be employed in the treatment of stricture, I venture to affirm that they have been and still are, grossly abused; that an untold amount of mischief is perpetrated by their abuse; that the routine employment of instruments in the treatment of stricture is in the highest degree unscientific and improper; that, in a word, in the treatment of structure, instruments of any kind should never be employed in any way except as a last resource. (Mr. W. S. Savory, p. 237.)

Urethrotome for Incising Narrow Strictures.—It is easy by the instruments hitherto used to incise a stricture which is not very narrow, but it is often these very cases which most require it. Mr. Berkeley Hill describes an instrument which is suitable for the purpose. The distance of the stricture from the meatus is first to be ascertained by accurate measurement. A guide bougie of fine whalebone, about one-fourth the size of a No. 1 catheter, is then to be introduced, and threaded upon this the urethrotome is to be passed. The incisor consists of a slender grooved sound of steel, with the ordinary curve, and provided with a tunneled end. In the groove runs this little rod having a blunt end fixed to it by a joint. When run along the groove of the sound, the jointed end and rod are quite concealed until they reach a point 6½ inches down the shaft. Here the groove becomes shallow and ceases; thus, the jointed end is thrown out and projects about one-tenth of an inch beyond the surface of the sound. It thus forms a little catch on the sound, which will travel along the urethra until it reaches the stricture, against which it will be caught. After the grooved sound has been passed along the guide, through the stricture, the little rod may be run down, and its end made to project into the urethra behind the stricture. By drawing forwards the sound thus armed, the posterior limit of the stricture is ascertained; the anterior limit is already known. The little rod is then withdrawn and replaced by another which carries, instead of a blunt point, a knife attached by a joint in the same way; this knife, concealed in the groove of the sound, travels down until it has reached the place where the groove grows shallow. This termination of the groove being already carried beyond the posterior limit of the stricture, the knife is thus made to project to a distance regulated by a screw at the handle against the floor of the urethra; and, if the sound be drawn forwards, the knife cuts its way until the whole stricture is divided. When the anterior limit of the stricture is reached, the knife is again brought within its groove, and the sound is withdrawn. The knife projects half an inch as a

maximum, but, by adjusting the screw, can be set to any less distance. (Mr. Berkeley Hill, p. 251.)

Maisonneuve's Urethrotome in Stricture.—Maisonneuve's urethrotome is probably the best instrument possible for the purpose for which it was designed. It is remarkably efficient and easy to work, and adapted for the narrowest strictures. The sound is of steel, hollow, split along the upper surface, and the size of a No. 2 catheter. The knife, of which I have two sizes attached to a thin flexible arm of steel, which bends as it slides along the concavity of the sound, cuts in advancing and receding, but only along its slopes, not at the free apex, which is knobbed and very blunt, blunt enough to leave uninjured the healthy part of the urethra. The lash-like appendage is a filiform bougie, fitted with a female screw at one end for attachment to a male screw at the tip of the sound. The bougie is first passed, its size being that of an English No. $\frac{1}{2}$ or less; then the sound is screwed on to it, and pushed *after* it, the bougie coiling up in the bladder. The knife is then run down the sound and withdrawn, the sound and bougie being pulled out next. The operation is easy, rapid, and successful, and produces neither more pain nor more bleeding than does 'divulsion.' (Mr. Rushton Parker, p. 254.)

Maisonneuve's Bougie Conductrice.—Some little care must be taken in the use of this instrument, for the blade may injure the healthy part of the urethra, and a guard has been used to prevent this possibility. It is also not safe to pass the knife until you are certain the instrument is in the bladder—this you can only be by inserting in the sound a stilet which shall fill the slit, so that when it is withdrawn, urine will flow if the bougie has gone into the bladder. As for 'divulsion,' 'dilatation instantanée,' 'immediate treatment,' or any other synonym by which the process of tearing open a man's urethra may be concealed, it has fully justified the almost universal condemnation passed upon it by Parisian surgeons. It has acquired the unenviable distinction of being the most fatal operation known for stricture, and I am aware of thirty deaths following its use by different surgeons. (Mr. W. F. Teevan, p. 255.)

The "Sonde à Vis, portant un Conducteur."—This instrument is the invention of M. Phillips, of Paris, and consists of an elastic catheter, and a flexible bougie. The bougie tapers away at one end to a fine line, whilst its other extremity enlarges to about the size of No. 2, and is fitted with a screw, which works into a socket on the vesical end of the catheter. The ends of the bougie and catheter are screwed together like the bamboo rods used

by the chimney-sweeps. The bougie is first passed into the bladder, and the catheter is screwed on to it, and then pushed slowly into the bladder, causing the whole of the bougie to enter that viscus, where its great flexibility allows it to coil up and remain quietly without the patient being aware of its presence till the catheter has effected its object of drawing off the urine. The entire instrument is then withdrawn, or the catheter can be unscrewed to leave the bougie *in situ*, to serve as a guide for the next occasion, or to dilate the stricture. The instrument is especially useful in cases of tortuous urethra from enlarged prostate or tumours of the bladder. (Mr. W. F. Teevan, p. 256.)

Retention of Urine Relieved by Ice in the Rectum.—M. Cazenave's plan is to plug the rectum with small pieces of ice. Mr. Teevan relates a case in which he adopted this procedure. In exactly twenty minutes after the last fragment had been inserted into the bowel the patient began to pass water guttatim, and in the course of half an hour contrived to empty his bladder. (Mr. W. F. Teevan, p. 248.)

To Relieve the Bladder in Bad Cases of Stricture.—Pass an exceedingly fine whalebone bougie through the stricture to the bladder, and upon this thread a fine silver catheter, having its end tunneled with an eye. (Mr. Berkeley Hill, p. 251.)

VENEREAL AFFECTIONS.

GONORRHŒA.—*Bromide of Potassium.*—Bromide of potassium augments the secretion of urine when administered internally, and renders it less irritating. This salt seems to have the power of diminishing the irritability of mucous membranes, not only when taken internally, but also when applied topically, for, according to Dr. Ringer, some writers aver "that merely brushing the pharynx and soft palate with a solution is sufficient to quell the irritability of these parts, so as to enable the laryngoscopic examination to be made with ease." Assuming such to be the case, it may naturally be inferred that a similar effect would be produced by its topical application to the inflamed urethra. For use as an injection, two drachms of bromide, with half an ounce of glycerine should be dissolved in eight ounces of water, and one syringeful used every four hours. When the disease has assumed the form of gleet, the bromide may still, with advantage, be given internally, but it should be combined with tincture of sesquichloride of iron. (Dr. J. W. Bligh, Montreal, p. 275.)

MERCURY IN SYPHILIS.—In the giving of mercury, blue-pill has had too much place. As a preparation it is bulky, and it tends when long given to upset the stomach. Syphilis is really (as shown by Mr. Hutchinson) a kind of fever, lasting, however, much longer than do ordinary fevers—sometimes even a lifetime. If, therefore, we are to benefit our patients, we must be content to give mercury in such a way as will least interfere with nutrition—to give it, therefore, in small doses, and over a lengthened period of time. In this way perhaps the liquor hydrargyri perchloridi, in drachm doses, is one of the best modes of exhibition, for this contains only a very small quantity of mercury, and, given in a vegetable infusion, is often of the utmost benefit. But in many cases there can be no doubt whatever that inunction, or the vapour-bath, suits better than any other mode of giving the remedy; and a paraphrase of the old rule—viz., the stomach for food, and the skin for mercury—might be held to be the best mode of solving the difficulty. (Editor of Medical Times and Gazette, p. 270.)

Although the inunction plan still enjoys undisputed pre-eminence at Aix-la-Chapelle (which may be considered as a sort of better-class lock hospital for the North of Europe), there is no reason whatever why it should not be given with as great advantage by the mouth. The inunction plan is dirty and inconvenient. The main rule in giving mercury in syphilis is to use small doses for a lengthened period. In order to secure the antidotal efficacy of mercury against syphilis, it is desirable to introduce a considerable quantity into the system, and to protract its use over a very long time. Ptyalism and other evidences of the physiological action of mercury, so far from being beneficial, are, if possible, to be carefully avoided, since they prevent the sufficiently prolonged use of the remedy. It is impossible to begin the administration of mercury too soon, and it should be resorted to without loss of time in all cases in which a chancre shows a tendency to indurate. It has not yet been proved that there are any special forms of syphilitic disease in which mercury ought to be avoided, although, as a general rule, it is acknowledged that it must be used with more caution in all forms which are attended by ulceration than in others. Iodide of potassium possesses little or no efficacy against either the primary or secondary forms of syphilis. The efficacy of mercury is often most signally proved in cases which have utterly resisted the action of iodide of potassium. It does not much matter whether mercury is given by the mouth, by

inunction, or by the vapour bath, provided that, whichever mode be selected, care be taken to avoid salivation, purging, &c. The doses usually resorted to for internal administration are for the most part too large, and thus often necessitate a premature discontinuance of the remedy. (Mr. J. Hutchinson, p. 267.)

Iodide of Potassium in Syphilis.—Syphilis is a disease more or less approximating to the characters of an eruptive fever, and tending like these to terminate of its own accord, with the evolution of certain symptoms. Mercury directly interferes with the evolution of these symptoms, cuts the various stages short and acts as a direct antidote to the specific poison which gives rise to the symptoms in question. In the tertiary stage of syphilis, the value of iodide of potassium is just as unquestionable as is the value of mercury in the earlier phases of the malady. It is a very important question for us to settle—if settle it we can—What is the value of mercury in this after-stage of syphilis? Suppose we see a patient with well marked rupia, with periostitic pains, and other signs of tertiary syphilis, who, nevertheless, has not taken mercury, what should we do? The first thing to note is that such symptoms, though usually sequelæ, may occur in the active stage of syphilis; and as long as syphilis is active, mercury will be of use; but as soon as the active symptoms have passed away, and the so-called tertiary stage begins, we must abandon the attempt to cure by mercury—we must give iodide of potassium. (Editor of Medical Times and Gazette, p. 272.)

Carbonate of ammonia greatly increases the therapeutic action of iodide of potassium. Five grains of iodide of potassium, combined with three grains of carbonate of ammonia, are equal to eight grains of the potassium salt administered in the ordinary way. The iodide may in the same combination be given in internal aneurism, and in the waxy liver of syphilis. (Dr. J. P. M'Sweeny, p. 274.)

AMPUTATIONS, DISLOCATIONS, FRACTURES, AND DISEASES OF THE BONES, JOINTS, ETC.

AMPUTATIONS.—*Safety of Torsion.*—Mr. Bryant reports that he has had only one case of secondary hemorrhage from a stump since he began the practice of torsion of arteries in January, 1868. This bleeding took place from the interosseous artery, to which he applied a ligature of carbolized catgut. There have now been at Guy's Hospital upwards of 200 cases of amputation of the thigh, leg, arm, and forearm, in which all

the arteries have been twisted (110 of these having been of the femoral artery), and no case of secondary hemorrage. (Mr. T. Bryant, p. 202.)

DISLOCATION OF THE HIP.—*Reduction by Manipulation.*—In a case of dislocation on to the dorsum ilii of seventeen days' standing, chloroform having been fully administered, I flexed the leg on the thigh, and the thigh on the abdomen, abducted the limb fully, and rotated outwards. During this last movement, I felt the tearing through of adhesions, and the head of the bone at once returned to the acetabulum. In a case of recent dislocation of the hip, flexion, abduction, and rotation inwards, resulted in transferring the head of the bone to the sacro-sciatic notch, whence it could easily be returned into the thyroid foramen. Having again removed it to the sciatic notch, I put my foot in perineum and made extension aid my movements, when it slipped at once into the acetabulum. It seems as if too great flexion was the cause of the bone slipping from the one position to the other without entering the acetabulum. (Dr. Hector C. Cameron, p. 107.)

BLOODLESS OPERATIONS.—The value of Esmarch's bandages for enabling the surgeon to perform operations with the parts unobscured by blood, is very great. In a case of acute caries of the middle third of the femur, chloroform having been administered and the heel well raised, I bandaged tightly, with india-rubber bandages two inches wide, from the roots of the toes, over the heel, to a point two inches above the intended seat of operation. While an assistant held his finger on the uppermost turn of bandage, the limb immediately above it was very firmly encircled with several turns of india-rubber pipe, about the thickness of the index-finger. The ends of the pipe, which was two yards long, having been firmly tied together, the elastic bandage was uncoiled, leaving the limb shrunken and waxy pale. A three-inch incision in the middle line down to the femur, free use of the gouge and chisel in the removal of a quantity of carious bone, an incision into a boggy spot inside the knee, and the passage of a drainage-tube through the opening thus made from the incision over the middle third of the bone, were the successive steps of the absolutely bloodless operation. (Mr. S. Gamgee, p. 137.)

The bloodless method of operating by means of the elastic band is a great convenience to the operator, as it makes him independent of sponges during the process, but it is questionable whether the wounds heal so quickly. It is necessary not to be in too great a hurry to close the wound after this method, but to wait a few minutes till the engorged condition

of the surface and edges of the wound have subsided, before applying the sutures, a matter which becomes of more importance after the larger operations, such as excision of the knee. (Mr. Cowell, p. 153.)

Mr. Heath has for a long time practised a method by which the limbs may be deprived of blood. The limb being raised, the house-surgeon draws his hand gently along the surface, so as to force back the blood to the trunk. A tourniquet is next applied, and a card placed beneath the screw of the instrument. After the operation, the tourniquet is gradually loosened, when the blood returns slowly to the limb. One great advantage to this plan is, that every surgeon already possesses all that is requisite for its accomplishment. (Mr. C. Heath, p. 143.)

Bandage the limb tightly from the toes to the upper third of the thigh, with elastic bandage about four inches in width, after which, work a stout elastic cord spirally over all, until it reaches the last turn of the bandage, which must then be removed. (Mr. J. K. Smith, p. 128.)

The following is the mode of preparing a limb for a bloodless operation:—A strong elastic bandage is applied in the ordinary manner to the extremity, firmly and slowly: a piece of india-rubber tubing of the diameter of the thumb is wound very tightly round the limb immediately above the margin of the bandage, which is then removed. The preparations are complete, and the limb is perfectly bloodless until the tubing is removed. The facility of operating under these circumstances can hardly be realised. The structures have a semi-transparency resembling that resulting from preservation in turpentine or glycerine, and as may be expected, they are picked out and distinguished with remarkable facility. The operations upon which Esmarch's method confers the greatest advantage are those in which much blood is ordinarily lost, or those performed under circumstances which prevent the operator from providing efficient aid. In cases where collections of septic matter exist Esmarch warns us against the danger of favouring by pressure its entrance into the circulation. The absence of bleeding points during the operation may cause us to neglect to secure some vessels which will subsequently be troublesome. In military and provincial practice, where, as already remarked, the surgeon must amputate without sufficient skilled assistants, he is free to select between the immediate danger of hemorrhage and the remote disadvantage of sloughing of the flaps. (Mr. J. E. Kelly, p. 138.)

The absence of hemorrhage is of peculiar value in operations upon bone, as the surgeon can see exactly what he is doing. Some arguments might be reasonably brought against the uniform adoption of this method; that though there were many cases in which patients could not afford to lose a drop of blood, yet there were others—for instance, in those of amputation in the thigh—where the sudden addition of so large a quantity of blood as that contained in the limb to the rest of the circulation might be more injurious than the loss of some part of it with the limb when removed. In operations for necrosis especially, this method greatly facilitated the manipulations of the surgeon. Previously the hemorrhage from the vascular structures cut through, so obscured the part that the sense of touch was the only guide to the operator; but now the surgeon could see the sequestrum, ascertain its extent, and easily distinguish it from its new bony casing. (Sir W. Fergusson, p. 105.)

Bloodless Operations by means of the Galvanic Cautery.—The capillary oozing of blood from the general surface of a wound, when the operation by which the wound was made has been performed after the bloodless method, may have some effect in preventing primary union. In aged subjects and in others in whom the vessels are diseased, sloughing or want of action is not unlikely to result, especially if the carbolic-acid spray has been employed at the same time. By means of the galvanic cautery we possess the power of performing many operations without loss of blood; and in comparison with this method all other so-called bloodless methods are delusions. But to secure this result much care and discretion are called for. The battery must be in good working order, and the instruments complete and whole: no make-shift will suffice in any way. If the battery does not work well, the required heat will not be maintained; if the instruments are imperfect, some break in the galvanic current may take place, and thus failure must ensue. As a matter of precaution, before operating, the surgeon should, therefore, test the battery and instruments that are to be employed, and in this way save trouble and prevent disappointment or, possibly, failure. There are no operations of importance that the surgeon has to perform which have been more benefited and simplified by the introduction of the galvanic cautery than those upon the tongue; for there are none in which, without its use, hemorrhage is more troublesome or dangerous, and there are none, with its use, which more satisfactorily illustrates its bloodless character. Indeed, before the introduction of the galvanic cautery or écraseur,

operations on the tongue were very rarely performed. For the details of the plan of operating for the removal of a cancerous nodule of the tongue, or of a cancerous tongue wholly or in part, we must refer the reader to the article, merely remarking, that what was formerly a very serious operation has, by this plan, become a very simple one. (Mr. T. Bryant, p. 129.)

The Elastic Ligature.—The division of parts by an elastic ligature is effected by the continuous pressure of the cord, which compresses the vessels and lymphatics until the vessels are plugged, and the access of nutritive material is completely prevented. Little or no pain and no febrile symptoms are caused. The mode of applying the ligature varies according to the peculiarities of the task to be performed. In nævus a single circular ligature is usually sufficient, and in two cases operated upon it fell off in eight days. In fistula ani or sinuses about the rectum, the elastic ligature has especial advantages where the inner opening of the fistula lies high up or the sinus extends far. In the case of sinus, an inner opening (into the rectum) is first made by means of a trocar. The trocar being withdrawn, the elastic thread is introduced through the cannula and drawn out through the rectum. This proceeding is rendered more easy by first introducing a metallic thread into the rectum through the cannula, seizing it with the finger or forceps, drawing it through the anus, and removing the cannula. The outer end of the wire is now fastened to the small elastic tubing by means of a waxed thread. In this way the elastic cord is very easily drawn through, if the index finger of the left hand can be passed up the rectum as far as the opening. Both ends of the ligature are drawn upon, and tied rather tight. The bridge of intestine with the sphincter is generally cut through in three or four days. The elastic ligature may be employed for the removal of tumours when they are pedunculated, or when they can be isolated, or when it is not possible or necessary to save the skin. The tumour may, if large, be tied in two halves. For this purpose a trocar should be passed beneath the tumour, and the trocar being withdrawn, two small pieces of india-rubber tubing should be introduced through the cannula. The elastic ligature may be applied to arteries. In one case the popliteal artery was tied. The ligature was thrown off on the sixteenth day, (Prof. Dittel, Vienna, p. 145.)

Removal *of Tumours by the Elastic Ligature.—Solid Elastic Cord.*—After experience with a fine india-rubber tube Sir Henry Thompson has come to the conclusion that solid india-rubber cord is much preferable, as it may be drawn at least twice as

tight, and therefore cuts more rapidly, especially as it is also much finer. The principal objection to the plan is the smell which necessarily arises from the sphacelated portion. This may be much controlled by irrigation with carbolic acid and water. No doubt a breast is a rather severe test for the ligature, although I do not hesitate to anticipate a very much better result with a more powerful cord. For the removal of the testicle, and for division of fistula in ano, I think it will be found admirable. The cord itself ought to be more accurately described. Its size before use is the following: [redacted] When applied it should be strained until it is a mere thread—say like this: [redacted] It would be very easy to devise a simple apparatus to tighten it subsequently, but it is so easy to accomplish this by pulling it out if loose, and tying a bit of fine ligature round the portion so drawn out, that it seems unnecessary to employ any other means. But with the new cord it is very probable that no such readjustment will be required. (Sir H. Thompson, p. 151.)

A woman, suffering from a large vascular tumour at the upper part of the vulval opening, presented a good opportunity for the employment of the elastic ligature, whereby the usual dangerous hemorrhage might be avoided. I ligatured the whole mass with a solid India-rubber cord two millimetres in thickness. I used this solid cord because the result of some previous experiments had satisfied me of the inadequacy of drainage-tube or any other variety of hollow caoutchouc tubing. The solid cord which I used is to be obtained at most India-rubber shops, the only difficulty being to procure it of sufficient thinness; when thicker than I have mentioned, it is unmanageable, especially for small growths. On the third day it became evident that the ligature was no longer acting, and on examination it was found to be quite loose. You may remember the simple way in which this difficulty was combated. A slender rod of ivory, one millimetre in thickness, was passed through the ligature circle and twisted round a few times, in the manner in which a ready tourniquet is made from a handkerchief tied round a limb with a stick twisted through it. These ivory rods can be had of various thicknesses in the shanks of ivory crochet-needles. Ivory is, I think, the best material, on account of its strength, lightness, smoothness, and rigidity; and by this simple means ligatural pressure can be kept to the very last to any degree of tightness required. (Dr. F. J. B. Quinlan, p. 210.)

EXCISION OF THE ANKLE-JOINT.—This operation is commonly performed by dislocating the bones of the leg inward. The

articulating surface of the tibia, however, is removed with much less disturbance to the surrounding parts by dislocating the parts outwards. The plan advocated, therefore, is to remove the internal malleolus first, and then the tibia and fibula may be dislocated outward through the external wound with great facility, and without interfering with any important structures. (Mr. Henry Lee, p. 120.)

Dr. P. H. Watson's Plaster-Splint after Excision of the Knee-Joint.—This splint is the best which has been hitherto invented for the after-treatment of excision of the knee-joint. It consists essentially of two parts: first, a suspension rod made of iron; second, a modelled Gooch splint, long enough to extend from the tuberosity of the ischium to beyond the heel. In application, the limb is first laid and carefully adjusted upon the posterior splint, which should preliminarily be padded with lint, and covered with gutta-percha tissue, or hot paraffin, in the situation which corresponds to the site of the operation. The iron rod is then placed in front, and folded lint laid between it and the limb at the groin (where the rod terminates above) at the upper part of the tibia, and at the bend of the ankle. The whole is then rendered immovable by means either of plaster-of-Paris applied by the hand, of a consistence like thick cream, or of paraffin, which, having been rendered temporarily liquid by heat is applied by a large paint-brush. When the application has solidified, the patient may then be removed to bed, and the limb suspended from the running pulley of a Salter's swinging cradle, or from the roof bar of the common iron wire cradle, employed to support the weight of the bed clothes. I do not regard it as a matter of indifference whether plaster-of-Paris or paraffin is used. Each has its advantages. The plaster-of-Paris is firmer and not liable to be affected by heat like paraffin, but it has the disadvantage of permitting soakage of discharge, it takes longer to consolidate, and when consolidated is less easily chipped through, by means of a bandage-shears, than the paraffin apparatus. (Mr. H. J. Tyrrell, p. 109.)

Fracture of the Clavicle.—M. Broca relates a case of oblique fracture of the clavicle, in which it seemed impossible to prevent the fragments riding over one another. He placed the arm in a semiflexed position behind the back, where it was retained by a bandage for eighteen days, with the effect of completely adjusting the fractured surfaces and producing an excellent cure. For a few days longer a sling was used. The patient, a man of considerable nerve, complained of the inconvenience and pain of the method for only the first twenty-four hours. (M. Broca, p. 125.)

FRACTURES OF THE PATELLA.—*New Apparatus for.*—The apparatus which I use is a hollow wooden splint, 4½ inches wide, and extending from above the middle of the thigh to the sole of the foot, at which point a footboard is attached by means of a hinge; this splint, having two transverse bars, is fitted into a long box-splint, the sides of which are forty inches long and six inches in depth, in which it travels horizontally. That portion of the splint on which the limb rests can be elevated or depressed as required, by means of perpendicular slots cut through the sides of the box-splint. Thus, the splint can be adjusted to suit a long or short leg, the limb can be elevated or lowered at pleasure, and the foot placed at any angle. This hollow splint is fixed in position by means of thumb-screws which fit into the transverse bars before mentioned; two semilunar pieces of metal, softly padded, are fixed one above the other below the fractured patella by means of leather straps which pass round the limb, the leg is secured to the splint by means of two broad web straps, one round the calf the other at the ankle; the foot can be bandaged to the foot board; a roller with each adjustment is fitted in the box-splint below the footboard. From this roller start four cords, which passing through brass sheaves, are attached, two to the upper and two to the lower metal pads by means of chains and light hooks. The roller is turned by means of a key, and acting on the cords, causes the metal pads simultaneously to approach each other, thereby bringing the fragments into apposition; the rack is covered by a brass box, which can be locked, so that the adjustment of the splint cannot be interfered with by the patient. Not only is this splint suitable for the fracture for which I have introduced it, but I believe also for fractures of the thigh or fractures of the leg. (Dr. W. J. Wheeler, p. 122.)

NERVOUS MIMICRY OF JOINT DISEASES.—The absence of swelling makes it very unlikely that a joint is really diseased; so does the presence of only a trivial swelling when the nervous and muscular signs of disease are acute or of long standing; and when swelling exists it must not be counted as adding to the probability of real disease, unless it persists. The sign most to be relied on for diagnosis between real and nervous disease of joints is the temperature. It is so important to estimate it accurately that I cannot too strongly urge you to be always studying it. You should feel with a broad surface of your hand every joint very watchfully, comparing each that is supposed to be diseased with its fellow supposed or known to be healthy, till you learn, as you certainly may, to detect even a small difference of tem-

perature in even a small part of a joint. A joint that feels all over cold, or cool, or not above its natural temperature, is not an inflamed joint: whatever may be the other signs of inflammation in it, it is not inflamed; you may rely on this. Sometimes a joint which feels quite cold, and is free from swelling, is reported as getting very hot every evening, and as being subject to painful heats. In any such case you may decide that the joint is not inflamed. The certainty of the diagnosis based on coldness is, if possible, increased by coincident dusklness of the skin—by its having the dull purplish tint which is commonly called blue or a dull pink. Such colours may be seen at joints long inflamed, but in these cases they are associated with over-heat; when they are associated with coldness, they are characteristic of anything rather than inflammation. (Sir J. Paget, p. 101.)

Removal of the Breast for Cancer.—In any case of cancer, the treatment must be very thorough; for any treatment, whether by the knife or caustic, which does not remove the whole of the disease, only does harm by stimulating the part left to increased action. In the case of the breast, it is, I believe, impossible for any surgeon to be sure that he has removed every germ of disease unless he extirpates the entire breast. I recommend you to make a very sparing use of the knife, and to trust very much to your fingers. You will find no difficulty, unless the patient is unusually thin, in stripping up the skin and its subjacent fat from the breast with your finger, aided by an occasional touch of the knife; and this plan has the advantage of securing you against slicing small portions of the breast or making "button-holes" in the skin. You can also better appreciate when you have reached the thin border of the breast, and so avoid leaving some of it behind, as is too often done. Forcibly raising the border of the breast, the surgeon at once ascertains whether the pectoral muscle is involved or not, and if it is, he should in my opinion, not scruple to cut into it freely, so as to remove, if necessary, a considerable portion of the muscle; and here again hemorrhage will be saved if the fibres are torn as much as possible. In removing the axillary glands, it is especially desirable to be chary of the knife, both on account of the hemorrhage and because the finger can hook them out so much more satisfactorily than any instrument. After completing the removal of a breast, I am in the habit of mopping out the wound very freely with the solution of chloride of zinc recommended by Mr. De Morgan, both on account of its antiseptic qualities and its possible effect upon

any cancer-germs left in the tissues. I used to employ it of the full strength (gr. xl. ad ʒ j.), but I fancied that it now and then caused the skin to slough, and I have therefore reduced it to one-half the strength, and I think, with advantage. In closing the wound, it is most important to leave a sufficient and dependant opening for drainage, and this is most conveniently placed near the axilla; and for a dressing I find nothing so satisfactory as oakum or carbolised tow, for the supply of real oakum has failed. This soaks up the discharge, and allows it to permeate, and hence keeps the wound dry, which I take to be the great point in favour of rapid healing and limited suppuration. It is convenient to place a piece of gauze between the skin and the oakum, so as to prevent the latter adhering, and to cover the oakum with a folded towel to absorb the moisture, which is abundant at first. (Mr. C. Heath, p. 39.)

Resin-Cloth in the Treatment of Wounds.—The carbolic gauze used by Professor Lister is most efficient for the purpose of treating wounds antiseptically. A much cheaper and equally efficient substitute may be made by saturating very thin calico in the following solution, after which it is to be pressed and dried. The exact composition of the solution is as follows:—Carbolic acid crystals, melted, two fluid-ounces; castor-oil, two fluid-ounces; purified resin, sixteen ounces by weight; methylated spirit, forty fluid-ounces; mix. To dissolve these ingredients easily, we must add them together in a certain order. To the resin, liquefied by heat and removed from the fire, add one-third part of the spirit, when these are well mixed, put in another third of the spirit, in which the oil has been previously dissolved; and, lastly, the acid in the remaining portion of the spirit must be slowly added to complete the mixture. The whole must be agitated until all the constituents are thoroughly incorporated, and afterwards passed through a muslin filter to get rid of any extraneous matters. If this plan be not adopted, the resin will concrete into a mass at the bottom of the vessel, and it will be extremely difficult afterwards to get it perfectly mixed. When thus prepared, the solution is of a dark colour, clear and free from any deposit; and it can be kept unchanged, in a well corked bottle for a long time. It is, in fact, a kind of thick varnish. To make the resin-cloth, as I term it, for the sake of distinguishing it from the cere-cloth dressing for wounds, which I described in a paper read at the Leeds meeting of the British Medical Association in 1869, it is needful to select a very thin, cheap, porous calico, or calico-muslin, known in the trade as "mulls,"

which costs at wholesale price about four shillings per piece of twenty yards. This, divided into strips, each about nine inches wide and six yards long, is reduced to a convenient form for general use. The castor-oil does away with the brittleness and adhesiveness of the resin. (Mr. E. Lund, p. 175.)

SELF-ADJUSTING SLING ARM-SPLINT.—Mr. Callender's sling splint for patients confined to bed with diseases or injuries of the arm is very useful. The apparatus consists of a wrought-iron support, a sling, and a counter-weight. The support is made of an iron bar bent at a right angle, the vertical part of which is round and works in a socket fixed by hooks or screws to the bed-head, so that it can be moved to the right or left readily by the patient when necessary. At the angle of the support is a small iron pulley over which the cord of the sling runs, and on the horizontal arm is another pulley, which can be moved backwards or forwards at pleasure. The sling is made of strong canvas, with four holes on each side, and supported by four cords with hooks, which can be so placed in the holes of the sling as to give the arm or hand any angle or position. These sling cords are attached to the single cord which passes over the pulleys, and has at its other extremity a bag containing shot, so that the weight may be increased or diminished as occasion requires. (Mr. H. H. Smith, p. 121.)

SILICA BANDAGE.—Silica bandages are now used in the Paris hospitals in place of gypsum and starch, and an exceedingly firm bandage results. The silicates of potash and soda, dissolved in an excess of caustic alkali, are now prepared in large quantity in the manufacture of soap, and can be easily obtained at a very moderate cost. Messrs. Hopkins and Williams, of 16, Cross Street, Hatton Garden, have supplied it in large quantities at 4d. a pound, and in small quantities at 6d. a pound, and it can be obtained, though inferior in character, at 2d. and 3d.,—so that it is not an expensive material. In fact, it is about half or a quarter the price of the thick gum solution used for similar bandages, and being firmer, does not require to be used in much more than half the quantity. The plan which I have adopted has been to encase the joint or fractured limb in cotton wool, lint, or a thin flannel roller; then over this to apply a common bandage, dry; over this to paint the silica by means of a brush or sponge; and to repeat the bandage and silica so that two or three layers of each exist. As soon as the last layer is dry, another coating of silicate is to be put on, so as to give an even surface. The limb is to be left exposed to the air for about half an hour,

but there is no fear of any of the silicate coming off after the first few minutes, and after half an hour or less (varying with the temperature) the bandage is firm enough to prevent movement. However, the bandage continues to harden for about two or three days, at the end of which time it should be quite firm; but it is usually firm enough in a few hours to insure immobility of a limb. We have therefore in this silicate bandage a means for providing immobility of joints and fractures which far surpasses in convenience, lightness, cleanliness, firmness, and cheapness any of the means hitherto made use of. If a limb be covered with cotton-wool, lint, wool, a worsted stocking, or any other soft protecting material, a surgeon may make use of strips of linen, bandage, or paper saturated with silicate of soda mixed or not with a salt of lime, such as chalk, whitening, or plaster of Paris, and procure a rapidly setting and extremely firm splint, the expense of which is reduced to a minimum. (Mr. W. W. Wagstaffe, p. 177.)

AFFECTIONS OF THE SKIN, ETC.

CARBUNCLE.—*Local Treatment.*—The use of carbolic acid so as, as it were, to saturate a carbuncle with it, whilst yet in its early stage, enables us in many cases to completely arrest its progress, and in all to modify it favourably. For this purpose, pass into the holes some threads of lint soaped in a strong solution of carbolic in oil (one part to four), and cover the hole with a piece of lint wet with the same, and let the application be renewed after a few hours. The carbolised lint must be carefully and scrupulously thrust to the very bottom of the small holes, a probe being used for this purpose. (Dr. P. Eade, p. 264.)

LUPUS, EPITHELIAL CANCER, AND NÆVUS.—*The Galvano-Caustic.*—One application of the galvano-caustic is in many cases sufficient to set up a new action in the part, and heal a lupus, destroy a cancer, or cure a nævus. It possesses advantages such as are not equalled by any other means the surgeon has at his command. It is applied with facility, certainty, and success: and it has rendered the treatment of these hitherto troublesome affections comparatively a simple matter. In some cases of nævi it is not necessary to destroy the whole. I have had several in which one of the eyelids was involved down to its ciliary border. In these I have been content with destroying half the growth, leaving the ciliary half alone; and I have been pleased to find that the ciliary portion of the disease subsequently disappeared, the redness gradually going, and leaving the skin white and

pliable. In extensive nævi, with these facts before us, it is consequently wise to deal with them piecemeal—to destroy a spot the size of a shilling at different parts of the growth; to cauterise the surface of the nævus when the skin is involved, and the deeper part by perforating it when the cellular tissue is affected; and in some cases it is well to make a clean sweep of the heated wire through the base of the growth;—the object under all these circumstances being the same—to destroy the vascular tissue, and excite sufficient inflammatory action in its deeper parts to cause what remains to wither. The larger and more extensive nævi may be thus treated with every probability of acquiring a successful result. (Mr. T. Bryant, p. 154.)

Onychia Maligna.—*Nitrate of Lead.*—In onychia maligna the last phalanx is usually double its natural size, livid red in colour, and ulcerated along the sides and upper edge of the nail. The pain attending the disease is most intolerable, and the treatment of it, hitherto, most unsatisfactory, even removal of the nail does not cure it. The loose portion of the nail should be removed by scissors, and powdered nitrate of lead freely applied to every part of the diseased surface every two days; the hard crust which forms is to be removed, and the powder re-applied. The effects are most satisfactory, all pain ceasing from one to three days after the first application, and a complete cure resulting. (Mr. Wm. MacCormac, p. 367.)

Dust the powdered nitrate of lead upon the onychia every night and morning, and at the same time administer cod liver oil and *steel wine*. (Mr. Howard Marsh, British Medical Journal, Jan. 17, p. 79.)

Suspected Ringworm.—The accurate diagnosis of suspected ringworm is very necessary, especially in the case of schools. These may be merely a "scurfy spot" upon which the hair appears to be growing fairly. The only real diagnostic test is furnished by means of the microscope, for if a few of the scales are scraped off and examined, they will always have little bits of diseased hair entangled in them where ringworm is present, and which diseased hairs are not perhaps visible to the naked eye. (Dr. Tilbury Fox, p. 259.)

Sycosis.—Sycosis is the common inflammation of the hair follicles of the beard and whiskers. Epilation is a fit procedure only at a certain stage of the disease, when the skin is much inflamed, the follicles freely suppurating and the hairs being thereby loosened in them. In the early stage of the case, a saline aperient, or aperient tonic, such as sulphate

of magnesia with sulphate of iron is advisable, at the same time hot fomentations and simple soothing applications, which exclude the air. When free suppuration has occurred, mild astringents such as zinc lotion and ointment should be applied. In the chronic stage, a course of Donovan's solution is to be given together with locally hot fomentations, and the application of a weak nitrate of mercury morning and night. (Dr. Tilbury Fox, p. 262.)

AFFECTIONS OF THE EYE.

A NEW PHOTO-PERIMETER.—Mr. Jeaffreson describes an instrument by means of which the limits of the field of vision of a patient can with rapidity and accuracy be transferred to a chart and kept for future reference. It will also test the field of vision for colours, and can be made to indicate the presence of astigmatism and show the meridian in which this exists if present. We must refer to the article itself for a description and diagrams of the instrument. (Mr. C. S. Jeaffreson, p. 279.)

AMAUROSIS.—*Hypodermic Injection of Strychnia.*—Dr. Taylor, of Nottingham, relates a case in which the patient had been nearly blind for six months, without any constitutional cause or cerebral defect to account for the loss of vision. On examination the disease was found to be simple white atrophy of the optic discs in both eyes, and the patient was at once injected under the skin of the brow, with the twelfth of a grain of nitrate of strychnia. His sight improved so rapidly, that he was enabled to resume his occupation as a moulder, at the end of about three weeks. The injection was repeated occasionally, but at what intervals of time is not stated. In another case, however, the hypodermic injections were used twice daily for five weeks. This mode of treatment does not always give such good results, but, as we cannot say beforehand whether it will succeed or not, and as it cannot be productive of harm, it should always be tried in cases of this nature. (Dr. C. Bell Taylor, p. 284.)

PURULENT OPHTHALMIA.—The treatment which has met with most success at the Royal London Ophthalmic Hospital is the frequent use of alum-water, generally six grains to the ounce of distilled water. Before doing this each time it is well to syringe out the conjunctival pouch under the upper and lower eyelid with tepid water; for this a common pewter squirt will do. At first, every second hour will not be too often, and then, as the ophthalmia yields, the intervals may be increased, until in a couple of days, six to four applica-

tions in the twenty-four hours will suffice. If the surgeon prefers the nitrate of silver, the eyelid should be everted and cleansed in the same way, then brushed with a solution of the salt—ten grains to the ounce. One such application a day is enough, and in the intervals a little alum-water, pure or mixed, with ext. belladonnæ may be dropped three or four times into the eyes. (Mr. J. W. Hulke, p. 286.)

MIDWIFERY, ETC.

AMENORRHŒA.—*Catelectrotonus of the Ovaries.*—Where the general health is as a rule excellent, the usual medicinal and surgical treatment of amenorrhœa often fails to rouse the dormant function of the sexual organs; and the various forms of electricity then step in as the right thing in the right place. The most effective mode of applying electricity I believe to be the induction of catelectrotonus of the ovaries. This is effected by placing the negative electrode of the constant battery alternately to the right and left ovarian region, putting the anode alternately to the lumbar spine and to the os uteri by means of an insulated sound. A current of from fifty to sixty cells of Daniell's battery was used, and the action of it kept up for fifteen minutes at a time. This treatment should be repeated periodically, just before the time when the flow ought to appear. The induction of catelectrotonus of the ovaries is a powerful remedy, by means of which, if perseveringly employed, the function of the ovaries may be restored even after having been in abeyance for years. (Dr. J. Althaus, p. 343.)

CHLORAL AS AN ANÆSTHETIC DURING LABOUR.—The more experience I have have had of the use of chloroform in labour, the less do I like it, and this, not because it does too little, but on account of its tendency to do more than we wish. In certain cases, when given with judgment, only during the pains, and not until these have become strong and forcing, it answers admirably, soothing the patient's suffering without retarding her labour or producing complete anæsthesia; in others, it has an unquestionable tendency to diminish the force and frequency of the uterine contractions. I know not what may have been the experience of others, but my own certainly is that in a large number of cases it has a very marked effect in diminishing the strength of the pains, and thereby very materially lengthening the continuance of the labour. Over and over again, when the administration of chloroform has been commenced, I have observed the character of the pains completely to alter, and again

recover their former efficiency as soon as the inhalation was suspended. Besides this, I have no doubt that a very continuous use of chloroform during labour has a marked effect in predisposing to post-partum hemorrhage, inasmuch as the tendency to undue relaxation of the uterine fibres continues for a time after the birth of the child. While, in my judgment, chloroform is apt to be too freely and incautiously used, the administration of chloral as a means of lessening the pains of labour is, I think, by no means as yet appreciated at its proper value. It has this immense advantage over chloroform, that it does not seem to diminish the strength and intensity of the pains, while it very markedly diminishes their painfulness. It has also another great recommendation, that it is chiefly applicable at a period when we would not think of administering chloroform—towards the termination of the first stage of labour, before the complete dilatation of the os, and when the sharp grinding pains perhaps produce more suffering and are less easily borne than the more forcing pains of a later stage. Hours and hours of really intense agony often elapse, until the patient is wearied and exhausted by her fruitless sufferings. In cases such as these, a common and very useful practice has been to administer a considerable opiate, so as to produce some hours of refreshing sleep, after which we expect the labour to recommence with fresh vigour and effect. The disadvantage of this plan, however, is that during the action of the remedy the labour is suspended, and much time is thus lost. If, however, chloral is administered instead of the opiate ordinarily employed, the probabilities are that the same refreshing rest will be obtained without any suspension of the pains or protraction of the labour. The character of the uterine contractions will be observed to alter; they will become steady and useful, but they are not suspended. Another condition frequently associated with the former is rigidity and spasm of the cervix. Very generally in this class of cases the cervix is thin and rigid, with a sharp edge. Soon after the chloral has taken effect the tissues seem to relax, and I have not unfrequently observed a thin os, which had remained unaltered in character for many hours, dilate rapidly under the influence of the remedy, far more so than when chloroform is inhaled for this indication. The way I give the drug is as follows:—I order a six-ounce mixture, containing a drachm and a half of the hydrate of chloral. When the pains are becoming severe, and I deem it advisable to employ the anæsthetic, which is generally not until the first stage of labour is approaching completion, I give one-sixth part of the mixture,—*i.e.*, fifteen grains of chloral. This I repeat in about twenty minutes;

and usually after the second dose enough has been taken to bring the patient sufficiently under the influence of the remedy. Its farther administration must now be regulated by its effects. (Dr. W. S. Playfair, p. 289.)

Chloral given during the first stage of labour has the effect of promoting the dilatation of the os, and at the same time it much deadens the acuteness of the suffering. A case is related in which three doses of thirty grains each were given at intervals of half an hour with the effect of producing a rapid and almost painless labour, the patient sleeping between each pain. (Mr. W. Berry, p. 292.)

HEMORRHAGE FROM ABORTION.—*Hypodermic Injection of Ergotin.*—Ergotin injected subcutaneously in two cases of severe hemorrhage occurring with abortion in the third month of pregnancy had an excellent hœmostatic effect. In each case that I adopted this procedure, hemorrhage had occurred of such magnitude as to completely prostrate the patient and imminently imperil life. Two injections in both instances were employed. In the first case, a grain of Bonjeau's ergotin in ten minims of distilled water twice, at an interval of seven hours prior to the expulsion of the ovum, each exhibition appearing completely to control the hemorrhage; and during the intermediary period there was no hemorrhage whatever. In the second, almost total collapse had accrued; hemorrhage had existed eighteen hours; the vagina had been carefully and most completely plugged; one ovum had come away and yet the hemorrhage continued. The pulse was imperceptible in both wrists; the stomach failed to retain even iced-water. Everything imaginable had been tried, when I injected subcutaneously, a grain and a half of ergotin in eleven minims of distilled water. Not fifteen minutes had elapsed before plug and a second ovum were forcibly extruded and hemorrhage immediately subsided. About eight hours subsequently flooding again commenced, owing most likely to relaxation of the uterus, and perhaps retention of some portions of membrane. Again I injected the ergotin, this time two grains, with equal success toward effecting control of the hemorrhage. (Dr. F. E. Clarke, p. 292.)

INFLAMMATION AND CONGESTION OF THE VAGINA AND OS UTERI.—The use of nitrate of silver is a mistake, and moreover, better results can be obtained by other means. Of all the agents which are applied to the vagina for the relief of inflammation or congestion of the canal, glycerine, without doubt, is the most valuable. A small roll of cotton-wool will absorb five or six drachms of glycerine; you fasten to this a strong thread or piece of twine, introduce it through

a speculum, and leave it in the vagina for twelve or even twenty-four hours, directing your patient to withdraw it at the expiration of that time by means of the string which is left hanging outside the vulva. Glycerine thus applied produces a copious watery discharge, which has a marked effect on the mucous surfaces in immediate contact with it. Thus, after its application the vagina and vaginal aspect of the cervix uteri appear pale, and the copious discharge seldom fails to relieve, for the time at least, that distressing sense of heat which is complained of in severe cases of vaginitis. In less acute cases the addition of ten grains of tannic acid to the ounce of glycerine often proves useful, but if used before the acute symptoms subside, it may cause increased irritation. Be sure whenever you use glycerine to warn your patient that she is to expect a copious discharge, otherwise the great flow which often comes on almost immediately will cause much alarm. (Dr. Lombe Atthill, p. 348.)

OVARIOTOMY.—No more favourable arrangement of circumstances can present itself than a solid ovarian tumour, freely movable, and surrounded by ascitic fluid; whilst on the other hand, a solid ovarian tumour, fixed and immovable, I invariably approach with the most serious consciousness of impending difficulty. It is easy, I think, to understand why this should be so. If a solid ovarian tumour be tied in position by adhesions, the probability is, that such adhesions will be pelvic and posterior; they will be situated at the very points at which it will be most difficult to separate them, and, even supposing this successfully accomplished, the raw surfaces produced will occupy the very positions which are fraught with the greatest after-dangers to the patient. I always hail the presence of ascitic fluid in the peritoneum as of highly favourable augury; and even adhesions of lymph, so only that they be soft and easily broken down, generally impress me favourably rather than otherwise. In the case of an unilocular cyst, I have long held with Mr. Southam of Manchester, that it is advisable, in all cases, to give the patient the benefit of tapping once, at all events, and that for more reasons than one. To tap a manifestly multilocular cyst is, in my judgment, to run a very serious risk, and one which is not generally counterbalanced by the amount of relief likely to be obtained by the operation; and hence it becomes a matter of considerable moment to be able to form a probable diagnosis as between the two forms. I desire explicitly to record my feeling in favour of a full and free primary incision. As to the best method of dealing with adhesions: if they be recent and soft, they

are easily broken down; and, following them carefully with the finger and the eye, to detect any vessel that may seem inclined to bleed, I generally so deal with such; but if the tumour be firmly adherent to the abdominal walls, or to any of the viscera, this cannot be done. If they be old and well organised, and pretty certain to live, I much prefer to cut out the portions of the cyst, and leave them attached, than to attempt to dissect them away. As to the best method of dealing with the pedicle, I am careful, above all things, if it be possible, to avoid having any drag on the stump; and when, having severed a tumour, and arrested all hemorrhage in the stump, I feel that I can safely drop it into the pelvis, and see it no more, I am the most easy about it. The two methods from which I have decidedly seen the happiest results have been "searing" and "torsion." The former is effected by seizing the pedicle near the uterus with a clamp, and then cutting off the tumour with the searing iron, and carefully burning away every atom of the eschar; the latter by simple twisting. The uterine portion of the pedicle, having been grasped by a clamp made specially for the purpose, is firmly held, and the end from which the tumour has been detached is enclosed in a second clamp, and the portion is then slowly and deliberately twisted off. In using either of these methods, I am still careful, when the clamps have been removed, to search carefully over the end of the stump, and if I see any bleeding point to tie it; and having done this, and assured myself that all is safe, I delight to feel that, henceforward, I have done with the pedicle. (Mr. C. G. Wheelhouse, p. 336.)

Immediately after making the incision through the walls of the abdomen the index finger should be passed up to the region of the umbilicus, and if it can be swept freely across from side to side it must be within the abdomen. This, of course, is an easy matter when no adhesions exist. It is always possible in parietal adhesions, when the finger is inside of the peritoneum. It is not possible, without the most unwarrantable violence, when the finger is between the layers of the abdominal parietes. The non-observance of this rule has led to the separation of large portions of the peritoneal layer of the walls of the abdomen, even when no adhesions existed, the operator having mistaken the peritoneum itself for an adherent cyst-wall. (Dr. Atlee, p. 342.)

Pelvic Hæmatocele.—In hæmatocele the blood is effused into the cavity of the peritoneum. The hæmatocele results either from the regurgitation of the menstrual fluid from the uterus

along the Fallopian tube into the peritoneal cavity, or else the afflux of blood which usually takes place to the generative organs at the time of menstruation leads to such an engorgement of the venous plexuses that rupture occurs at some point where either a varicose or other diseased condition existed, which predisposed to the catastrophe. In a certain number of cases—but they are, in my experience, very rare—the hemorrhage occurs in connexion with pregnancy, or rather with delivery, either at term or, more commonly, prematurely, and especially during the earlier months. It will almost invariably be found that symptoms of ovarian or uterine disease have existed in these cases prior to the occurrence of the hæmatocele. The diagnosis has to be made from pelvic cellulitis and pelvic peritonitis. Pelvic hæmatocele is characterised by symptoms which are more sudden, severe, and alarming than either of the other two—it, in fact, shows its traumatic character very distinctly; and pain, prostration or collapse, with symptoms of internal hemorrhage, are its leading features. The pain is of course very local and limited, and it has a peculiar forcing, bearing-down character, a feeling which is described sometimes as if everything were being forced away. There are differences in the character of the swelling. In hæmatocele it is at first tense, elastic, fluid, and fluctuating; as time goes on, it becomes boggy, doughy, non-fluctuating; then firmer and firmer, till it is almost hard, and with this change it contracts considerably, getting smaller and smaller, till it finally disappears. (Dr. Alfred Meadows, p. 354.)

Post-partum Hemorrhage.—No one should go to a case without a catheter, secale, opium, ammonia, a good syringe, and (as far as my present experience has shown me its value) the perchloride (or a kindred preparation) of iron. If we have reason before the birth of the child to suspect that post-partum hemorrhage will occur, a good dose of secale should be given just before the head comes on the perineum. Should flooding occur, the uterus and vagina must be cleared of all clots, and firm pressure externally maintained. By attending to the actual condition of the uterus after the conclusion of the labour by means of the hand pressed upon it gently through the abdominal walls, we ought to be forewarned of hemorrhage and act accordingly. The next thing to try, supposing the hemorrhage continue, is cold externally, and if this fail, a jet of cold water carried up to the fundus by means of the syringe, or passing a piece of ice into the uterine cavity. If these means fail the case is a very desperate one, and only two measures of treatment remain to us. The first

is, pressure upon the aorta and vena cava, by means of a firm pad pressed well down upon the vessels, the patient being of course upon her back. The second, and only remaining resource is injection of a styptic into the uterine cavity, such as perchloride of iron. In my own practice, I employ a rather weak solution at first—one of the weaker liquor ferri perchloridi to six or eight of water. If that fail, then I make it stronger; but, if the flooding be rapid, I use the full strength of the weaker solution or the tincture, because much of it is lost in the blood, or is prevented from coming easily into contact with the walls. The tube of the syringe should be carried to the fundus, and the fluid gently injected; or it may be conveyed with a sponge, which should be passed over all the internal surface. Perhaps this is the safest mode; but it must be removed promptly, or the uterus may retain it, as occurred in one case I know. I have tried it in all kinds of *post partum* hemorrhage, after removal of clots, which is important, or they become coagulated and firm, and cannot be removed afterwards without trouble. I have found it particularly satisfactory in cases of irregular and intermittent relaxation, causing the uterus to assume its proper form at once. (Dr. Braxton Hicks, p. 310.)

Cold Water Injections.—Having introduced the hand to ascertain that there is no retained fragment of placenta, grasp the fundus as firmly as possible, and at the same time inject cold water freely into the uterus. For this purpose the long tube of an ordinary stomach pump, or enema apparatus, is to be employed and passed well up into the uterine cavity. (Dr. W. Boyd Mushet, p. 305.)

Perchloride of Iron.—We have three stages of hemorrhage to deal with. 1. There is hemorrhage with active contractility of the uterus. Here the diastaltic function may be relied upon; excitants of contraction find their application. 2. There is the stage beyond the first, when contractility is seriously impaired, or even lost. Here excitants of contraction are useless; our reliance must be upon the direct application of styptics to the bleeding surface. 3. There is the stage beyond the first two, where no remedy holds out a hope unless it be transfusion; and even this will probably be too late. The practical directions are as follows:—Take a Higginson's syringe, to which is connected a uterine tube nine or ten inches long. Mix in a deep basin four ounces of the strong liquor ferri perchloridi of the British Pharmacopœia with twelve ounces of water; pump through the delivery-tube two or three times to expel the air; then pass the delivery-tube into the uterus, so that its end touch the fun-

dus of the uterus; then pump gently and slowly; the styptic fluid will thus bathe the whole inner surface of the uterus. (Dr. R. Barnes, p. 306.)

I always carry the perchloride of iron with me, and, if any excessive loss occurs, use it as directed by Dr. Barnes in his work on "Obstetric Operations." I believe the satisfactory results which I have met with are due to my using the solution promptly, and in not waiting till the patients were in a state of collapse. For the purpose of injecting it, I use a common syphon syringe, to which an ordinary vaginal flexible nozzle is attached. This should be carried fairly up to the fundus of the uterus. I generally find that four or five ounces of the solution are sufficient. As to the strength, Dr. Barnes recommends that four ounces of the strong liquor ferri perchloridi be added to twelve of cold water. I generally use it stronger—about two parts of water to one of the liquor. One precaution only is needed, namely, to take care that the tube is passed well into the uterus before any of the fluid is injected; otherwise the vagina will be corrugated and the os uteri contracted from the effects of the iron, the styptic will not reach the interior of the uterus, and great difficulty will be experienced in any subsequent efforts to introduce the tube. The same reason, namely, the effect produced by the action of the styptic on the vagina and os uteri, renders the application of the iron by means of a sponge difficult and unsatisfactory. The great injury to which steel instruments are liable, when kept in a bag which also contains a solution of the perchloride of iron, has induced me to carry the salt in the solid form, the bottle containing it being enclosed in a boxwood case. (Dr. Lombe Atthill, p. 308.)

Danger from Injections of Perchloride of Iron.—No reasonable objection can be urged to the employment of these strong styptics, provided the one essential condition be attained—the complete and permanent contraction of the gravid uterus. For it appears a matter of little consequence by what means the contraction is produced; so long as it be complete and permanent, as to close the arteries, and obliterate the canals of the veins or sinuses, and prevent any injurious absorption, the woman is safe, and will make a good recovery. But the danger in the use of these agents, as shown by the fatal cases which have followed the employment of them, appears to consist in only partial contraction being produced, by which although the hemorrhage was arrested, yet the veins or sinuses remained open, and permitted the perchloride of iron to be taken up and conveyed into the general system. The con-

sequence of this, as shown by the fatal cases, has been that symptoms of blood-poisoning supervened in the course of a few days, ending in the almost inevitable death of the individual. It seems desirable to have some agent always at hand, and incapable of producing such results. Ordinary vinegar in the proportion of one part to two or three parts of water has been tried in some six or eight cases with very encouraging results. In some of the cases however, in which styptics, such as perchloride of iron, have been injected, there was not the least occasion for it, seeing that firm pressure externally, with or without the introduction of the hand into the uterus had not even been tried. (Dr. T. Snow Beck, p. 316.)

After the completion of a natural labour, the uterus becomes sufficiently relaxed to allow the blood to escape from the open ends of the torn arteries. The means which are adopted to induce the contractile tissue to again contract and close the canals of these vessels, either fail to provoke contraction, or only induce partial contraction; and in either case the hemorrhage continues, when a solution of the perchloride of iron, of varying strength, is injected into the cavity of the uterus. After the injection of the perchloride, the hemorrhage ceases at once, or very soon afterwards; the patient revives; and all things appear to be going on favourably till about the third day, when the usual series of fatal symptoms commence by the woman complaining of feeling very ill. The symptoms, once begun, continue gradually to increase in gravity, and at the end of two, three, or four weeks, the patient appears to fall asleep as death closes the scene. The whole mischief in these cases of *post partum* hemorrhage appears to turn upon the single point of the want of proper contraction in the gravid uterus; and the sole question in the treatment upon the best means of inducing and maintaining complete contraction. Want of contraction, or subsequent relaxation, which practically is the same thing, allows the blood to escape from the arteries; want of proper contraction permits the canals of the sinuses to remain pervious, the noxious fluids to enter them, and death to result. When complete and permanent contraction is induced, the woman appears to be safe from all ill consequences, and will make a good recovery. But it is also known that even the use of the perchloride is sometimes not followed by any perceptible contraction; or by only partial contraction, by which the flow of blood is arrested, and the way to injurious impregnation of the general system left open. (Dr. T. Snow Beck, p. 324.)

SPINA BIFIDA.—*Injection of Iodine.*—Dr. Watt relates a case of spina bifida cured by injection of Dr. James Morton's solution of iodine and iodide of potassium in glycerine. Two tentative tappings with a medium cannula, of respectively twelve and ten ounces of clear fluid, were made, with an interval of eight days, both being followed by an irritable and feverish condition of the child, and gradual refilling of the tumour to its original size. Ten days afterwards, other ten fluid-ounces were removed, and half a drachm of the solution injected. The opening was instantly closed on each occasion by a layer of lint soaked with flexile collodion. Ten days afterwards eight fluid ounces were removed, and a similar quantity of the solution injected. This was followed by severe irritative symptoms, but a fortnight later the tumour was only one-third of its original size, and a repetition of the treatment at that time resulted in a cessation of any further formation of fluid—a roughened, darkened, thickened condition of skin replacing the tumour, and closing the spinal aperture with an apparently gelatinous mass. (Dr. J. Ross Watt, Brit. Med. Journal, Jan. 31, p. 137.)

SPONGE TENTS.—Sponge tents are far safer than those made of sea-tangle, and it has long been one of my objects to remove the very offensive smell after their use. I have tried charging them with various disinfectants, but without any result, until last week I made an experiment with oil of cloves, and I have found that a tent charged with 5 per cent. solution of oil of cloves will remain in the uterus for twenty-four hours without becoming offensive in the slightest degree. There can be no doubt that such a tent is far safer than those ordinarily in use, and it is certainly much more agreeable to the operator. Messrs. Krohne and Sesemann, of Duke-street, W., make these tents according to my formula. (Mr. Lawson Tait, p. 360.)

TRANSFUSION OF BLOOD.—If a sufficient amount of blood is not procurable, a solution of phosphate of soda, carbonate of soda, and chloride of sodium, at a temperature of 100° F., should be injected first, as one factor in the danger attending these cases of prostration is the nearly dynamic one arising from the absence from the heart of fluid to act upon. The blood which is procurable may be injected afterwards. Blood may be used either whole or defibrinated. The choice will be governed by circumstances. If we have Aveling's or other proper apparatus to practise transfusion from arm to arm, so that the blood passes direct from giver to receiver, without ever being exposed to the air or allowed to rest, this will often be the preferable plan; but where the proper

apparatus is wanting, as it is likely to be, defibrination is better. So long as it is considered necessary to have a special apparatus, the operation must be performed rarely. But let it be understood that blood either whole or defibrinated will do, and that almost any syringe can be made to answer, and the great obstacle to the extension of the operation will have vanished. Nothing can be more easy than to furnish every stomach-pump case with a flexible tube to which a transfusion-canula is attached. As no practitioner of medicine ought to be without a stomach-pump, the *necessary* apparatus will never be wanting. I should not hesitate to use an ordinary Higginson's syringe. (Dr. R. Barnes, p. 330.)

VAGINAL INJECTIONS.—The solutions of alum and of sulphate of zinc so frequently used often aggravate the case when vaginitis is present. They coagulate the albumen of the leucorrhœal discharges, producing a number of hard masses, which often cause much discomfort. Borax is a better agent, but it sometimes causes irritation, though in a less degree. A drachm of borax to the ounce of water is the proper strength. Where the object is to soothe and to allay irritation this is best effected by the use of an infusion of tobacco. Tobacco must, however, be used with caution. Some patients are peculiarly susceptible of its action, especially those in whom the orifice of the vagina being narrow some of the fluid is retained in the canal. Begin, therefore, by infusing fifteen grains of the unmanufactured leaf in a pint of boiling water. If this produce no unpleasant effect increase the strength to thirty—even sixty, grains to the pint. In many cases the addition of a drachm of borax to each pint of the infusion greatly increases the efficacy of the treatment. Many patients, however, are unable to use the tobacco at all, as even a very weak infusion causes nausea and faintness. I therefore recommend, when this is the case, or where you fear to run the risk of causing any discomfort, to substitute for tobacco an infusion of hops, directing an ounce of hops to be infused in a pint of boiling water with or without the addition of borax, as you may deem advisable. (Dr. Lombe Atthill, p. 348.)

VASCULAR GROWTHS OF THE FEMALE URETHRA.—*Application of Chromic Acid.*—Concentrated chromic acid has the two great merits of very efficaciously destroying the growth, and at the same time being much less painful than potassa fusa or nitric acid. The use of the actual cautery is out of the question unless chloroform is given. My usual plan is to roll a little cotton-wool round the end of a small piece of wood (an ordinary match will do); to soak it for a quarter

of an inch in the acid solution; and then to carefully yet firmly press it upon the vascular growth until the surface becomes distinctly shrivelled; the surrounding parts being guarded by cotton-wool soaked in solution of carbonate of soda, which is also applied to the growth subsequently, to neutralise any excess of the acid. Slight burning pain is produced, but seldom sufficient to need any employment of morphia, either hypodermically or as a local application. (Dr. A. W. Edis, p. 333.)

MISCELLANEA.

BROMIDE OF POTASSIUM.—Dr. Reynolds speaks of KBr as if it were the only remedy worth consideration in the direct treatment of simple *epilepsy*. He says: "It is to be demonstrated, in my opinion, that there is something 'specific' in the action of KBr. Potassium—given as iodide—is without such effect; and bromine—given as bromide of ammonium—has no obvious influence on epilepsy; but in combination, these two elements—bromine and potassium—are of undoubted value." And he sums up the general question as to epilepsy thus:—"(1) The cure of epilepsy is effected by doses varying, for the adult, from 5 to 40 gr., given three times daily. (2) It is not the mere administration of the drug, but its presence in certain quantity, that is necessary for a cure. I would earnestly recommend that bromide of potassium should not be discontinued in the treatment of a case of epilepsy because of its apparent failure; but that the dose should be gradually increased, and the exhibition of the drug most patiently carried on for a period of many months or even years." Elsewhere Dr. Reynolds remarks that, while thus remarkably efficacious in *epilepsia gravior*, the bromide scarcely at all effects the attacks of *petit mal*, and is also less efficacious than usual where the convulsive attack only occurs at night. Cases of "cure" in the ordinary acceptation of the term, *i.e.*, in which well developed fits had been stopped completely, and had remained absent long after the discontinuance of the medicine, are, however, perhaps never seen. On the other hand there are numerous instances in which very long periods of freedom have been obtained. In spasmodic diseases like *false croup* and *asthma*, we have the very high authority of Dr. W. Begbie for the statement that bromide of potassium is a powerful remedy; and this is backed by the testimony of Ringer, who also adds that it

is most efficacious in the colic of children, and also in the night terrors of young children. The latter writer expresses the following opinion as to its effect in convulsive diseases:—"Although convulsions may be excited by many causes, it is probable that the conditions of the nervous centres producing the attack are in every instance identical; and it appears to be these conditions which the bromide controls." (Dr. Anstie, Practitioner, Jan. 7, p. 19.)

COCA.—*Its Therapeutic Use.*—It is possible that this drug may turn out to be of considerable value in cases of exhaustion. The leaves are derived from the *Erythroxylon coca*, a shrub that is indigenous in South America. They are chewed by the Indians, who are stated by Mantegazza, Moreno, Guibourt, and others, to go through laborious work for several days and nights in succession, without exhaustion, although they have little or no food, provided that they have a supply of about eight ounces of coca-leaves to chew during the day. An infusion of half a drachm to a cup of hot water is said to produce beneficial effects. (Dr. E. H. Sieveking, p. 363.)

CROTON-CHLORAL.—Croton chloral, unlike the hydrate, dissolves with difficulty in water, and crystallizes in small glittering tablets. Its action differs widely from that of chloral hydrate in some important particulars. Four *grammes*, or a drachm, of this substance, dissolved in water, and introduced into the stomach, produce in the course of from fifteen to twenty minutes a deep sleep, accompanied by anæsthesis of the head. Whilst the eyeball has lost its irritability, and the nervus trigeminus shows no reaction whatever on being irritated, the tone of the muscles remains unaltered. In some cases of tic douloureux, the remarkable phenomenon is exhibited that pain ceases before sleep sets in. I am sorry to say, however, that this remedy acts only as a palliative in this dreadful disease. I nevertheless prefer its action to that of morphia, because it has effects as good as the latter remedy, without being so detrimental to the constitution in general. I have never observed any unfavourable effects of croton-chloral on the stomach or any other organ, although I have made frequent experiments with it. The indications for the use of this remedy are to be found—1. In cases where hydrate of chloral is inapplicable on account of heart disease; 2. In cases of neuralgia in the district of the nervus trigeminus; 3. In cases where very large doses of chloral are necessary to produce sleep. I there recommend the addition of croton-chloral to hydrate of chloral. (Dr. Oscar Liebreich, p. 361.)

the commencement of anæsthesia, which should afterwards be continued with pure ether. (Dr. T. G. Hake, Practitioner, April, p. 241.)

Raising the chin, and pulling it as far as possible away from the sternum, generally prevents the movements of deglutition, which are excited by the administration of chloroform, and which prevent its free inhalation. It will be found on trial that swallowing cannot be accomplished with the chin considerably elevated. Spasmodic closure of the glottis may occur, when chloroform or ether vapour of too great pungency is given. Should this occur, the chin should be well raised, and, if necessary, the tongue drawn forwards by forceps, which are to be kept ready for that purpose. Ether is undoubtedly safer than chloroform, but it is far from being really safe. Most English surgeons are in favour of giving the ether slowly for about a minute or so at first; and then, when some degree of anæsthesia has come on, to give the vapour strongly. I am decidedly in favour of this plan, and have sometimes succeeded in getting a patient to sleep without any struggling. The most pleasant way is to give nitrous oxide till consciousness is lost, and then administer the ether of moderate strength. Next to nitrous oxide, I think chloroform is the least unpleasant anæsthetic to breathe. Patients who have previously taken chloroform are often unwilling to try nitrous oxide, because they have no objection at all to the former; but I have rarely found a patient who had once been put fully under the influence of ether, who was willing to inhale it again, unless it had been preceded by nitrous oxide. I admit that chloroform has more deaths to account for than ether has; but we must not forget that chloroform is much more frequently administered than ether, and that coroner's inquests are not held in countries where the practice of giving ether instead of chloroform is prevalent. Still there are greater dangers from chloroform than from ether, although I do not think them so insidious that every one who inhales it runs even an infinitesimal chance of losing his life. In whatever way the chloroform is given, the condition of the patient's pulse, as well as his breathing, should be carefully watched all the time he is inhaling; and, when the breathing is deep or the pulse weak, the chloroform should be diminished or removed. In the administration of anæsthetics generally, but especially in giving chloroform, it is advantageous to have the stomach empty or nearly so. I like to give a teaspoonful of brandy, without water, a few minutes beforehand, but not so much as a tablespoonful. If wine be given, or if the

patient must have some water with the brandy, then they should be given half-an-hour before inhaling, to allow time for their absorption. A patient should have his pupils insensible to light, or breathe stertorously, before the commencement of a severe operation; but one or two inspirations of pure air should be taken before the anticipated shock is given. I now very rarely give bichloride of methylene from choice, having less confidence in it than chloroform. In one case the respiration and pulse ceased, and artificial respiration had to be resorted to. I have used a mixture of alcohol, one part; chloroform, two parts; ether, three parts; and like it much, for cases where moderate narcotism has to be kept up a long time—*e.g.*, when caustic chloride of zinc has been applied to a cancerous growth. I have tried, in a few cases, tetra-chloride of carbon, ethyliden chlorid, and most of the other anæsthetics that have been recommended; and the conclusion at which I have arrived is, that they are neither safer nor more pleasant than ether and chloroform. Nitrous oxide causes sleep without any delirium or excitement. It is by far the best anæsthetic for many short operations, such as the extraction of teeth, opening abscesses or boils. It answers very well in operating for strabismus. Removal of the eyeball was performed lately by Mr. Bowman for a lady, who said she had no consciousness of the operation. It is well suited for examining hysterical cases, wrenching stiff joints, and reducing luxations of recent date. I do not think it suitable for cases where it is necessary to keep a patient quiet more than three or four minutes; but, if the patient be allowed to recover consciousness after one inhalation before another is commenced, the anæsthesia may be kept up tolerably well for half an hour. In administering nitrous oxide, a plentiful supply of gas is essential to success. There is no fear of patients inhaling too much at first; indeed, they should be told to breathe deeply and slowly. Nitrous oxide may be used with advantage to prepare the way for giving ether; but I would not advise it before giving chloroform, because, firstly, chloroform is not very unpleasant if given gradually, and, secondly, it would be more dangerous to give chloroform in the state of excitement from gas than without it. (Mr. Joseph T. Clover, Brit. Med. Jour., Feb. 14, 1874, p. 201.)

A New Expedient in Administering Chloroform.—Herr Heiberg, of Christiania, has hit upon an expedient in administering chloroform, which in his opinion is available for the avoidance of danger from imperfect respiration with rattling and lividity, which is liable to occur during the administration of chloroform. This consists in drawing forward the under jaw *in toto*. When the rattling, incomplete respiration begins

—that is to say, in all those cases in which the teeth are otherwise forced apart, and the tongue drawn out—I draw the under jaw forwards by the following means:—Standing preferably behind the reclining patient, the operator places both thumbs on the symphysis of the lower jaw, presses the second joint of the bent forefingers behind the posterior margin of the rami ascendentes of the under-jaw, and thus holding the whole bone fast between the two hands, draws it forcibly forwards (anatomically speaking). The most successful impulse is that which would be given if the intention were to lift the whole head and body by this grasp. By this proceeding and as long as it is continued the jaw is kept luxated forward. The obstacle to the respiration is removed, and, in short, exactly the same result is obtained as if the tongue had been drawn forwards and the mouth kept open by a gag. (Herr Heiberg, Medical Times and Gazette, Jan. 10, 1874, p. 36.)

RICHARDSON'S TOOTH-EDGE CUTTING SCISSORS.—These scissors are intended for the removal of tumours, and for general use in any operation where it is desirable to avoid much hemorrhage. They are particularly suitable for cases of epulis. They are of the ordinary construction in all respects except in the cutting edge. The cutting edge of each blade, instead of being even and sharp, is divided into finely-pointed teeth, each tooth being directed with a slight inclination towards the handle of the scissors. When the blades meet the teeth cross each other, and as they pierce any structure that may lie between them, they crush also, between their surfaces. If a piece of moderately firm substance be placed between the blades—a piece of paper or of thin card, for example—the scissors perforate it in a series of perforations resembling what is seen in the postage stamp—that is to say, they do not cut clean through the substance so as to leave it in two distinct parts at once. A little lateral or half-rotating movement of the closed blades is, however, sufficient to tear through the still connected lines of substance and to complete the separation. The same occurs if the substance placed between the blades be a portion of soft animal structure, only that more force is required in the lateral or rotating movement to cause complete separation. The parts punctured are crushed between the teeth, and are separated by the twist or torsion. I find these scissors useful in dividing directly and quickly structures in which there are many minute blood-vessels, and which, when divided by the knife, bleed freely. These toothed scissors, as they can be made at one and the same time to pierce, crush, and twist, control bleeding remarkably. (Dr. B. W. Richardson, p. 363.)

SULPHIDES.—*The Therapeutic Use of.*—The sulphides of potassium, sodium, and calcium are therapeutic agents of value in cases of suppuration. When they are administered, a thin, watery, unhealthy discharge becomes at first more abundant, afterwards diminishing, and becomes throughout thicker and healthier. The condition of the sore improves correspondingly, and its healing is promoted. The sulphides appear often to arrest suppuration. Thus in inflammation threatening to end in suppuration they reduce the inflammation, and avert the formation of pus. This effect is manifested when sulphur compounds are employed locally in acne indurata. The advantage to be derived from these agents is especially seen in cases of slow suppuration in the glands of the neck in children, such as follows scarlet fever. In some of these cases the system is worn out by the pain and constitutional disturbance. If a tenth of a grain of sulphide of calcium, mixed with a grain of sugar of milk, is given in in such a case every hour, or two hours, the results are most striking. The swelling becomes smaller, the pus reaches the surface in four or five days, and when it is evacuated leaves a benign wound which quickly heals. The effects of these remedies are equally conspicuous in mammary abscesses, although in rare instances they appear temporarily to increase the pain—a remark which seems sometimes to hold good with respect to boils. In boils and carbuncles these remedies yield excellent results. A tenth of a grain of sulphide of calcium, given every two or three hours, generally prevents the formation of fresh boils, while it lessens the inflammation and reduces the area of the existing boils, and quickly liquefies the core, so that its separation is much more speedy, thus considerably curtailing the course of the boil. Where the skin is not yet broken, and the slow-separating core therefore not yet exposed, the sulphides often convert the boil into an abscess, so that on bursting pus is freely discharged, and the wound at once heals. The topical effect of sulphur ointment, or of an ointment of the hypochlorite of sulphur, or, still better, of the iodide of sulphur of the Pharmacopœia, is most marked in acne indurata and acne rosacea. Here, again, the effects are twofold, and even opposite, according to the stage of the eruption. If applied at the very commencement of the eruption, as soon as the little hard knot is felt under the skin, further development is arrested, and the hardness speedily disappears. If rubbed over the skin it appears to check the formation of the acne spots. If rubbed over the nose and neighbouring parts of the face in acne rosacea its effects are often striking. Not only does it act as in acne

indurata, but the hardened, swollen tissues become softened and reduced to a more natural state. (Dr. S. Ringer, Lancet, Feb. 21, 1874, p. 264.)

THERMOMETER. — *Patent Self-Registering.* — Mr. Casella, of Holborn-bars, has succeeded in constructing a most useful modification of the clinical thermometer, the idea being to prevent the oft-recurring difficulty in clinical thermometers which renders them practically useless—viz., the escape of the index into the bulb, where, mixing with the bulk of the mercury, it is irrecoverably lost. The instrument is constructed in precisely the same way as the ordinary clinical thermometer, with this important exception—viz., that the capillary tube issuing from the bulb is gradually contracted for a distance of half an inch, and then suddenly expands into an oval chamber or trap, beyond which the tube is continued of the usual diameter to the extremity of the instrument. The thermometer supplied to us, which we have had in use and found most efficient, has, whether by accident or intent, a contraction in the tube both above and below the chamber or trap. This, without being a disadvantage, provides a still further security against the destruction of the index, for it is almost impossible to shake it even into the trap or what may be called a secondary bulb, much less into the all-engulfing bulb. Mr. Casella guarantees this instrument from becoming disarranged by any amount of violence, whether in the hands of the physician or the most inexperienced person. We can strongly recommend this thermometer as superior to any we have seen or used.—(Medical Times and Gazette, Feb. 28, p. 252.)

TOBACCO SMOKE.—*Effects of.*—M. Gustave Lebon, of Paris, has recently issued a treatise upon the Chemical and Pathological Effects of Tobacco Smoke upon the Human System, the author having first constructed a variety of apparatus by which he has been enabled, he states, to collect and show with exactness the various elements of smoke which condense and deposit themselves on the organs of the smoker. One hundred grammes—1,300 grs., English troy—of French tobacco furnish 0.550 grs. of nicotine and 0.490 grs. of ammonia. Tobaccos of the Levant are the least dangerous, some of them possessing but a small trace of nicotine. In small doses, he says, tobacco smoke instantaneously excites cerebral activity and the intellectual forces, and facilitates digestion. In excessive and frequently repeated doses, it produces difficulty of digestion, benumbed intelligence, and clouded memory. (M. Lebon, Medical Press and Circular, Dec. 24, p. 575.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON TISSUE-DESTRUCTION IN THE FEBRILE STATE, AND ITS RELATIONS TO TREATMENT.

By Dr. FRANCIS E. ANSTIE, Physician to Westminster Hospital.

[It has hitherto been considered a maxim in pathology that the persistence of the febrile state at a certain high standard necessarily involves a large consumption of the nitrogenous tissues of the body. This idea is founded upon the fact of the large amount of urea passed in urine, notwithstanding the diminution or even entire cessation of nitrogenous food supplies.]

Now the extended inquiry which I have been lately carrying on has shown, in a very surprising manner, the possibility of a total absence of urea-increase during the course of an intense pyrexia of considerable duration. That is the broad fact, in the natural history of the state, which has come out: I now proceed to give details.

Before entering into the question now raised, it is needful to have distinct ideas of what constitutes a genuine urea-excess. The first thing necessary to know is, what is the average urea-discharge of health.

Now it is obvious, in the first place, that the amount of urea will vary enormously according to the character of the diet. It would be impossible otherwise to account for the exceedingly various estimates of the health-range of urea-discharge which have been given by excellent observers. In Parkes's work on the urine, twenty-four authors are quoted whose estimates were all made on males between the ages of twenty and forty; the figures vary from 286·1 to 688·4 grains for the twenty-four hours. The mean of the whole gives an estimate of 512·4 grains urea-discharge daily. But in Senator's recent and most valuable work on pyrexia I find, with surprise, that he quotes a statement of Unruh, which would make the normal discharge in men as low as 270 grains (17·466 grammes) in

twenty-four hours. It is quite certain, however, that unless the individuals from whom this was taken were feeding on a diet unusually poor in nitrogen, some abnormal circumstance must have been present.

The second point to be remembered is that the concurrent results obtained by many observers show that in healthy adult women the daily urea-discharge does not exceed three-fourths of the quantity excreted by healthy adult men similarly circumstanced. According to our modern physiological ideas it would be easy to account for the whole of this difference by the inferior quantity of azotised food consumed by females.

The third point to be remembered is, that in health the urea-discharge is the result, in by far the largest extent, of the decomposition of nitrogenous food within the body.

In the febrile state, on the contrary, there is usually such complete anorexia that the supplies of nitrogenous matter taken are very small, yet the urea-discharge is commonly far higher than in health; sometimes double the amount, or more. Not only in case of a positive increase of urea under these circumstances, but even when it sinks some way below the normal level, we may still conclude (from the small nitrogenous food supply) that exaggerated tissue destruction has largely to answer for its formation. The question is, can the urea-discharge be so far depressed below the normal rate, in any considerable number of cases of decided pyrexia, as to indicate decidedly that nitrogenous-tissue destruction is taking but little part in the actual urea-formation? The cases which will be brought forward in the present paper certainly tend, as far as they go, to answer that question in the affirmative.

Typhoid Fever.—In the case of this disease a large body of evidence has been collected, of late years, which appeared to show, with great uniformity, that when the fever was at all severe (as measured by continuously high temperatures, together with any marked delirium, the urea-discharge was constantly above the normal level, at a time when a very small amount of azotised food was being taken. For example, Dr. Parkes, besides speaking very definitely, from his own experience, in favour of a regular increase (save in certain abnormal conditions) of urea beyond the normal level (one-fifth on the average), also quotes three important series of observations, by Moos, Brattler, and Warnecke, extending altogether over 78 cases. The significance of the figures will not be perceived unless we remember that the patients were on exceedingly low diet—lower than we should employ in England. Moos (from eighteen cases) gives the average daily discharge as 575·64 grains in first week, 517·92 grains in second week, 404·04 grains in third week, 343·2 grains in fourth week. Warnecke gives,

from results of fifty cases (thirty males and twenty females), for *men* an average discharge of 673·92 grains in first week, 517·44 grains in second week, 482·04 grains in third week, and 361·92 in fourth week; for *women*, 530·4 grains in first week, 471·12 in second week, 375·96 grains in third week, 319·8 grains in fourth week. Brattler gives (from ten cases) 607·4 grains daily in first week, 592·8 grains in second week, 421·2 grains in third week, 327·6 grains in fourth week, 247·6 grains in fifth week. The near approximation of these different series of results is very remarkable. Nevertheless, Parkes himself points out some objections to their conclusiveness; and my own results will be found to fully justify a revision of the usual belief. That is to say, they will show that the exceptions to the rule are probably so numerous as to form an important subject for consideration in regard to fever doctrines and fever treatment. The histories now to be given are of patients in whom neither stimulants, nor any important medicine were given during the observed period.

The first case which I shall relate is that of Charles Jefferson, aged 25, a small but well-nourished man, who was admitted into Westminster Hospital on the seventh day of typhoid fever, and upon whom the first rose spot was seen on the following day. The examination of the urine unfortunately could not begin till the ninth day. At this time there was no diarrhœa: but I may mention here that even at the period when diarrhœa did come on we did not lose urine from that cause, as the patient intelligently obeyed our orders to pass water into the chamber-vessel before going to stool. This circumstance shows, of course, that the case was not of the severest type. Full convalescence was established upon the twenty-fourth day, as marked by the cessation of diarrhœa; the temperature had become normal two days earlier. I had intended to print in diagrammatic form the respective curves of mean daily temperature, of urea elimination, and of water elimination, but there was unfortunately no time for this on the present occasion, so the figures must be verbally stated. On the ninth day the temperature, though not quite so high as it had been, was nearly 103° on the mean of three observations, the maximum being a good deal higher: in striking contrast with this was the fact that only 118 grains of urea were eliminated. During the five following days the temperature had steadily come down and the urea had as steadily gone up; the twenty-four hours' mean temperature on the fifteenth day stood at 100·5, while the urea had risen to 385 grains. Here, unluckily, the patient became mutinous; for although he had diarrhœa, and slight delirium every night, he felt so well and hungry in the day-time that he was greatly disgusted with his rigid diet. I had kept him strictly

the commencement of anæsthesia, which should afterwards be continued with pure ether. (Dr. T. G. Hake, Practitioner, April, p. 241.)

Raising the chin, and pulling it as far as possible away from the sternum, generally prevents the movements of deglutition, which are excited by the administration of chloroform, and which prevent its free inhalation. It will be found on trial that swallowing cannot be accomplished with the chin considerably elevated. Spasmodic closure of the glottis may occur, when chloroform or ether vapour of too great pungency is given. Should this occur, the chin should be well raised, and, if necessary, the tongue drawn forwards by forceps, which are to be kept ready for that purpose. Ether is undoubtedly safer than chloroform, but it is far from being really safe. Most English surgeons are in favour of giving the ether slowly for about a minute or so at first; and then, when some degree of anæsthesia has come on, to give the vapour strongly. I am decidedly in favour of this plan, and have sometimes succeeded in getting a patient to sleep without any struggling. The most pleasant way is to give nitrous oxide till consciousness is lost, and then administer the ether of moderate strength. Next to nitrous oxide, I think chloroform is the least unpleasant anæsthetic to breathe. Patients who have previously taken chloroform are often unwilling to try nitrous oxide, because they have no objection at all to the former; but I have rarely found a patient who had once been put fully under the influence of ether, who was willing to inhale it again, unless it had been preceded by nitrous oxide. I admit that chloroform has more deaths to account for than ether has; but we must not forget that chloroform is much more frequently administered than ether, and that coroner's inquests are not held in countries where the practice of giving ether instead of chloroform is prevalent. Still there are greater dangers from chloroform than from ether, although I do not think them so insidious that every one who inhales it runs even an infinitesimal chance of losing his life. In whatever way the chloroform is given, the condition of the patient's pulse, as well as his breathing, should be carefully watched all the time he is inhaling; and, when the breathing is deep or the pulse weak, the chloroform should be diminished or removed. In the administration of anæsthetics generally, but especially in giving chloroform, it is advantageous to have the stomach empty or nearly so. I like to give a teaspoonful of brandy, without water, a few minutes beforehand, but not so much as a tablespoonful. If wine be given, or if the

on a mixed diet, approaching as fast as he safely can to the food of a healthy man. As soon as he shall have become fairly established on that, I will report on the amount of his normal urea-discharge. He is a hearty eater when in health.

The broad features of the above case are: (1) That the water elimination and the urea-discharge closely coincided in amount, but that they ran a *precisely opposite course to that of the mean temperature* during the whole period between the ninth and the fourteenth day. [It will be seen, hereafter, that even the water and the urea by no means always correspond in amount.] (2) That the average urea-level was exceedingly low, for it might well have been expected (according to former estimates) that such a patient would have excreted a daily average of 500 grains during the second week of typhoid, whereas he actually passed only 300 on the average. (3) I may now add that the nitrogenous matters introduced as food (in 80 ozs. milk) proved on analysis to yield 113·5 grains nitrogen, or the equivalent of 227 grains urea: so that it will be seen that but a small portion of the urea-discharge needs to be accounted for by destruction of proteinous tissues. (4) In correspondence with the slight degree of muscular destruction which took place in this case was the remarkably small degree in which muscular strength was affected. This was repeatedly pointed out by me to the students; and the same fact has been noted in several other cases.

Just before this case occurred, another patient, Mary Hughes (aged 32, single), had also been in Westminster Hospital suffering from typhoid fever. The past history of the case was very confused. It was impossible to say whether, at her admission, she was already in the fourth week of typhoid fever, whether she had had a "relapse," or whether she had passed into typhoid after suffering from some other febrile affection. At any rate she was in a condition of very high pyrexia, the temperature fluctuating between 104° and 105°, or even more for the first two days, and crops of well-marked rose spots appeared daily for five or six days after her entrance to the Hospital. She had no diarrhœa at any time; but this is a feature which has been exceedingly common for some months past, even in some of the best-marked cases of typhoid at Westminster Hospital, and, I believe, elsewhere. She had no delirium, no appearance of anxiety, and although she lay in bed her muscular strength seemed scarcely at all affected. It was curious to see this in a patient the dry burning heat of whose skin would have led one to expect a very profound degree of prostration. From January 24th to February 2nd inclusive (nine days), the total nitrogen of the urine was daily most carefully estimated by my friend Mr. Frederick Hicks (formerly assistant to Professor Sir Ben-

jamin Brodie), by the soda-lime combustion process as modified by Parkes. On several of these days I myself examined, for urea only, by Liebig's method; and the difference between our figures was only so large as corresponded well enough with the probable amount of nitrogen contained in the excreted lithates uric acid, creatine, creatinine, &c. The diet was rigidly confined to 60 oz. milk (containing a little over 85 grains nitrogen) during the first two days, for the other seven it consisted of 40 oz. milk, and 20 oz. best beef tea (containing nearly 120 grains nitrogen). The nitrogen discharge was as follows:—105 gr., 115 gr., 149 gr., 103 gr., 87 gr., 116 gr., 77 gr., 44 gr., 170 gr. As it has been suggested to me by one or two friends that cases with a low urea-discharge may perhaps void a large amount of nitrogen through the skin, I may remark that at any rate this patient did not perspire *fluid* at all considerably, nor did the man Jefferson.

The daily volume of water eliminated by the woman Hughes corresponded somewhat, but not closely, with the fluctuations of the urea. The figures (in cubic centimetres) are: 512·5, 570, 590, 500, 710, 635, 700, 700, 980. From this it can only be said in general terms that both urea and water were low; but it will be seen that, *e.g.*, while on the seventh and eighth days of observation the volume of water was exactly the same, the nitrogen was 77 grains on the former and 44 on the latter day: and many other discrepancies could be pointed out. The above nine days were the only ones on which the total nitrogen was estimated; but for three or four days longer (I should mention that temperature became normal two days after the last combustion-research) I watched the urine, by Liebig's process, on the chance of a great rush of urea and water elimination, but nothing of the sort occurred. The urea slowly rose up to what appeared (on mixed diet) to be about the woman's normal discharge, viz. 350 grains. This being probably her standard in health, it is very remarkable to observe the low ratio of elimination during the nine days first mentioned, and especially during the first four of them, when the temperature was *never below* 103° at any part of the twenty-four hours. During these four days the urea-discharge did not average more than 238 grains; while the ingoing nitrogen averaged 102·5 grains, equivalent to 205 grains of urea. And it is interesting to observe that this evidence of a but slight destruction of the azotised tissues is fully in agreement with the woman's remarkable conservation of muscular power.

It will immediately occur to some, as a criticism on the above case, that the urea-discharge might well be low, seeing that the patient had been continuously or nearly continuously febrile for a month when our observations began, and the available

albumen of the blood and tissues might have been used up by a copious urea-discharge in earlier stages of the disease. But it seems difficult to suppose that anything of the kind had taken place; for in such case there must have been extreme muscular weakness, whereas the very reverse was the fact. And indeed, supposing that we regard the above as a case of typhoid really at the end of the fourth week at the time of admission, the best evidence hitherto obtained (including Parkes's) goes to show that when the fever keeps up at this late stage the urea-discharge keeps up also. The remark may also be made, in anticipation of a similar criticism on Jefferson's case (in which the observations were not commenced till the ninth day), that his entire freedom from muscular prostration was inconsistent with the idea that there had been any such enormous combustion during the first week as might have temporarily used up the nitrogenous matters available for elimination. Since writing the above I have analysed Jefferson's urine, first day of *full diet*:—Chlorides normal; urea, 503·88 grains.—*Practitioner*, *March*, 1874, *p.* 193.

2.—JURGENSEN ON THE SLIGHTER FORMS OF TYPHOID FEVER.

By the EDITOR OF THE MEDICAL TIMES AND GAZETTE.

Typhoid fever must be a subject of interest to the medical profession for many years to come, until its history is more fully worked out than at present, and we are able to say with absolute certainty what is and what is not typhoid. It is still a question whether many of the cases now returned as simple continued fever are not in reality typhoid; and we cannot but be indebted to all those who, by their observations, aid in settling this important question. There can be little doubt that a form of typhoid fever which, from its mildness, does small harm to the patient himself, is still quite as dangerous, *quoad* infectiveness, to the community at large as one labouring under a more severe variety of the disease; and it certainly makes all the difference to the patient himself, whether his doctor recognises the nature of his illness or not, and treats him accordingly. The philosophical and, in many ways, original lecture of Professor Jürgensen, of Tübingen, on the slighter forms of typhoid (Volkmann's *Sammlung Klinischer Vorträge*, No 61), of which the following is but a brief abstract, will therefore be doubtless acceptable to most of our readers:—

There are several reasons why the slighter forms of typhoid fever have not received the attention they deserve. In the first place, medical men are too apt to consider the representative

descriptions of a disease given in books as portraying actual forms of that disease, whereas they only exhibit at one view the aggregated characteristics of many varieties of it. Thus, when they find a case which does not fit into the typical description, they disregard it altogether, to the manifest detriment of the patient.

Again, slight forms of fever may not come under the physician's notice at all. They are called by the patient's friends "rheumatic" or "catarrhal," and receive domestic treatment.

Thirdly, many abortive forms of disease can only be diagnosed from *etiological* considerations, and to do this a physician must see many patients. Now, during large epidemics his time is necessarily devoted to the severer cases, as every man's strength has certain limits. Moreover, mild cases as a rule do not seek admission into hospitals.

Professor Jürgensen lays down the following dictum, which must be accepted if our present views about the acute exanthemata be correct—namely, that in each of them, since each arises from a specific cause, we must be able to find cases which shall represent every stage of intensity of that disease from the slightest to the most severe forms. By a specific cause is meant a something (*Etwas*) with properties peculiar to itself, which occur in it alone in all nature. This "something" produces, by its entrance into man's organism, certain peculiar and sharply defined anatomical and functional phenomena in individual organs and in the general processes of nutrition (*Stoffwechsel*).

There are three categories of infectious diseases—(1) those in which the person attacked can by contact with another produce in him the same disease in turn—*e.g.*, the acute exanthemata; (2) those in which a carrier of the infectious principle is required for it to enter the body from without—*e.g.*, typhoid fever and cholera; (3) those which require the aid of factors external to man's organism in order to become efficient—*e.g.*, malarial affections.

Now, the conditions of the existence of an infectious disease are that the "something" (*Etwas*) spoken of above shall meet with an organism in which mutual action and reaction (*Wechselwirkung*) can take place. The exact form (*Erscheinungs form*) in which the disease then manifests itself depends clearly on two factors—the intensity of the exciting poison, and the resisting power of the host,—and both vary very widely; so that these considerations alone would lead us to expect all grades of severity of such a disease. The patient himself will be the gainer by this view, for when the physician comes to consider the sick man's organism and its resisting power

as one of the two factors which he must keep in view, he will be less likely to forget that he has a diseased person, and not the entity called disease, to treat (*einen Kranken, keine Krankheit zu behandeln hat*).

When two bodies, mathematically speaking, react on one another—*cæteris paribus*,—the effect produced depends on their relative size. We must therefore inquire whether the *intensity* of an infectious disease varies with the *quantity* of the poison received into the system or not, and so whether the existence of slight forms of the disease means the absorption of small amounts of the infecting material.

We can adduce several arguments in favour of such a view. Thus, epidemics vary in intensity as well as in extensiveness, and we may say in general terms that the most extensive epidemics are equally the most intense. Or (2) take an individual epidemic. The worst cases occur when it is at its height; and is this not most simply explained by the presence at the time of a large quantity of the poison? In the latter case objection may be made that in zymotic diseases there is a multiplication of the poisonous element in the organism itself; but the objection is less trustworthy than it at first sight appears. Jürgensen and Körner have proved that pus from a soft chancre can be diluted with successive additions of an indifferent liquid (*e.g.*, serum), so that, in inoculations with it, not only is the incubative period gradually lengthened, but the size of the resulting pustule diminished until it cannot with certainty be diagnosed as chancroid. Here the result is probably due to tissue changes, by which some of the germs are destroyed, for Ludemar Hermann has especially pointed out that the effectiveness (*Wirksamkeit*) of a poison depends on its concentration in the blood, and this is being continually lessened by the processes of excretion from the blood.

Some persons may consider that the cause of difference between the intensity of different examples of the same disease lies in a change in the *quality* of the poison received. It has been decisively proved that vaccine matter loses its power when transmitted through many generations of men, probably from the destruction of some of its germs through tissue-change; or else it may be that while the number of the germs remains the same, their properties have altered through time. Whatever view they take, all persons must admit that at any rate *a greater or less amount* of the poison can act so as to produce disease; so that this alone would account for its slighter forms, even if the resisting power of the receiving organism were to remain the same. The question next arises, Is the *resisting power* of different individuals really the same?

and in answering it we must clearly distinguish between the *receptiveness (Empfänglicheit)* of an organism and its resisting power to the poison when received. *Receptiveness* to chancre-poison, for example, will depend on whether the skin of the penis is thick or thin, whole or excoriated, in two individuals; but the poison once absorbed, the one with the thicker skin may have severer symptoms than the other. This may explain the fact that some people hold out longer against the invasion of a disease than others, and also why cholera often follows dietetic errors. *Resistance* to disease must be broken up into two factors—(1) general resisting power—*i.e.*, a good constitution, about which we can make no certain prediction prior to experience; (2) resisting power to certain toxic substances, a factor of excessive variability in different individuals. This is shown by the effects of alcohol, opium, and the alkaloids, and is true, also, of the class of infectious diseases which most people only suffer from once in their life, though a few may have them twice or more. For instance, Jürgensen knows a person who has had scarlet fever four times, the two last times when the disease was only sporadic. On the other hand, how many escape such diseases—for example, scarlet fever—altogether; and what would have happened at the time of the Plague if everyone had had it? So, then, since we find such variations in the grade of intensity of the poison, and in the receptiveness of the individual, we must admit that all gradations of intensity of the disease from zero to the maximum are possible, and to be expected.

We can now turn to the phenomena of the slighter forms of typhoid fever as Jürgensen describes them, and see in what they differ from the ordinary forms. It was Griesinger who first called attention to such cases, and showed that it is their *short duration*, and not the slightness of a typhoid fever running its normal course, which characterises them. Although his facts have not been doubted, yet they have not received the assent they deserve. Jürgensen has founded his present statements on the observation of more than a hundred cases during three epidemics in and about Kiel in 1865, 1866, and 1868. In the last epidemic, twenty-nine persons connected with the academical hospital were attacked, and one died and was found to have all the anatomical characteristics of typhoid fever. There was a close resemblance between all the cases. In the first place, the onset of the disease is usually sudden (seventy-four times in eighty-seven), and in half the cases is accompanied by a pretty well-marked rigor. There is headache, while the temperature rises quickly to its maximum—sometimes to 40° Cent. (104° Fahr.) in thirty-four hours,—and not in the usual zigzag fashion. At the height of the disease morning remis-

sions of 1° to 1½° and nocturnal exacerbations occur, just as in the ordinary severe forms, only the remissions are slighter. The absolute height of the temperature varies considerably, and the influence on it of quinine and of cold applications is relatively great. After cold baths there are often very decided irregularities in it—*e.g.*, higher morning than evening readings. In the second stage, a high temperature with only slight remissions is not so very uncommon, the thermometer standing above 40° Cent. for several days. The fever-curve of the slightest forms of typhoid exhibits the influence of insufficient infection in all their stages "by the prevalence of the law which governs the normal temperature over that called into operation by the typhoid poison." The usual steep curves of the third stage (high evening and normal morning temperatures) fail entirely in the slight forms, and the temperature generally falls gradually in from twenty-four to seventy-two hours to its normal level.

Swelling of the spleen was present in eighty-one out of eighty-eight cases. Roseola is often absent in the very short cases (ten days); but generally present in those lasting two weeks or more, and occurs about the fourth or fifth day; and as a general rule, the more intense the rash, the more severe the disease. Bronchial catarrh seldom occurs, and diarrhœa was only present in 16 per cent. of Jürgensen's cases, while perforation and hemorrhage from the bowels were not observed once. Albumen is often found in the urine early, especially in the cases in which the fever runs high. The period of convalescence is relatively a long one, and is accompanied by great muscular weakness, which may even pass on to temporary paralysis if the muscles be at all over-strained. Relapses are rather of frequent occurrence, though Jürgensen refers them all to errors in diet on the patient's part, as he found a definite connexion between the two in all his cases. He believes that the so-called "*Typhus ambulatorius*" of books is only a slight attack of typhoid fever, prolonged and exacerbated by dietetic errors, and he therefore insists even more strongly on absolute diet in the slighter than in the ordinary well-marked forms of the disease. No patient under his care is allowed to take solid food until the temperature in the rectum has not, for at least six days, reached 38° Cent., four readings being taken daily. Jürgensen's explanation of the typhoid relapses after improper food is that in such patients the intestine does not at first offer a suitable nursery (*Boden*) for the increase of the fever poison, but requires an external irritation—*i.e.*, solid particles of undigested food—to fit it for reproduction. Therapeutically, in addition to absolute diet and rest in bed, Prof. Jürgensen makes use of quinine and cold baths in such cases as there seems danger from the fever process itself.

The same season of the year (August to November), and the same period of life (fifteen to thirty years of age), which furnish the greatest percentage of severe typhoid cases, also give the greatest percentage of the slighter.—*Med. Times and Gazette, Feb.* 14, 1874, *p.* 188.

3.—ON THE ANTIPYRETIC ACTION OF QUININE.

By Dr. CLIFFORD ALLBUTT, Physician to the Leeds General Infirmary.

[Fever is a disturbed balance, and like other disturbed balances of a moving equilibrium, it tends under favourable circumstances to resolution. Many fevers arise from a single interference, which is not repeated, and the initial velocity and the subsequent course of the disturbance depend upon the character of this single interference. It is a general rule, that disturbances having a high initial velocity are sooner expended than those in which it is lower.]

This is not the time to sum up carefully what is known of the effects of pyrexia when thus considered, but I may shortly say that they fall into two chief categories, those seen in discharges, or in oppression of the nerve-centres, and those seen in greater combustion of tissues. This excessive combustion is strongly marked in the muscular tissues, not excluding the heart. Shortly, then, violent or prolonged pyrexia threatens life through the nervous system and through the heart. The degree of violent pyrexia which will kill is tolerably well known, but clinical experience has yet to decide what averages of daily aberration are compatible with the continuance of life for a week, a fortnight, a month, and so on. This ought to be made out. It is certainly true, however, that small gains in daily temperature are very large gains in time, and that as we descend the scale in daily temperature, we get largely multiplied increments of immunity. Thus an aberration of 107° is soon fatal, an aberration of 106° is alarming, an aberration of 105° with slight remittances is tolerable for some days, an aberration of 104°, with similar slight remittances, may be borne not only longer but very much longer, and aberrations of 103° may be borne for a month or more. If there be more oscillation, if, say, the daily temperature fluctuate between 100° or 101° and 103°, the combustion may be tolerated for very long periods, certainly for ten weeks, and perhaps for more than that time. But we need far more careful estimates of this, and such estimates must be made on so large a number of similar patients as to eliminate, as far as possible, the error of varying individual resistance.

Does an artificial diminution of the degree of the fever on one or on several days modify the after-course of the fever

either in its duration or in its average intensity? Observers seem fairly agreed that in fevers which have a definite course it does not so modify it, and in other fevers it is hard to form a judgment. In the use of any antipyretic agent we have therefore to expect a temporary alleviation only. We cannot shorten the period of the whole aberration, nor can we flatten its trajectory, so to speak, except by a continuous application of our means. We know tolerably well, then, what our aims must be, namely, to stop superheating in the diseases in which this may occur, and in fevers of long periods to flatten the curve so as to bring it within those parallels which experience tells us are safe for its calculated duration.

Let us now endeavour to apply these reasonings. In typhus, scarlatina, pneumonia, and rheumatic fever, the pyrexial danger, is generally of the brief and intense variety; in typhoid it is more often seen in the longer continuance of a more moderate daily average; in septic fevers it is seen in aberrations which although often high, yet so alternate with much lower degrees as to lessen the mean daily temperature, and thus to make them very tolerable to the system. A septic fever, again, is very often added to a specific fever, and may thus either modify its course or may prolong the pyrexia when the more definite movement is expended. Such an event is to be seen in the absorption of septic matter from the throat in scarlatina, from the bowel in typhoid, from the lung in pneumonia, from the skin in small-pox, and so forth. Such septicæmias, whether complicating or supplementing the course of a specific fever, must be carefully distinguished if our antipyretics belong to two equally distinct and correlative classes.

Before the action of quinine as an antipyretic can be interpreted and rationally applied, we must have something like accurate answers to all the foregoing questions. For my own part, I have nothing to offer but the crude impressions derived from a wide but fragmentary clinical experience. In typhoid fever I have used quinine for about fifteen years, being first incited thereto by Dr. Fuller, at a time when Dr. Fuller was testing the quinine treatment of that fever proposed by the late Dr. Dundas. Soon I extended the use of the drug from that to all other febrile states, and have thus arrived at certain opinions rather than conclusions, which may have some temporary value in the absence of any better results from others. Speaking generally, I have found quinine to be a very powerful antipyretic in septic fevers. In that kind of remittent pyrexia which is seen in pyæmia, in septic absorption, in erysipelatous peritonitis, and the like, I have generally found that quinine in daily quantities of 20 to 60 grains will greatly reduce the oscillations. I have very often found, indeed, that by its

means the rises may be wholly prevented, and the patient made apparently fever free. Again, in the prolonged hectic of pulmonary disease and like affections, I have often found it easy to moderate the daily movements, and perhaps to prevent them; but in such long-continuing cases it is not desirable to add chronic cinchonism to the other symptoms, and five or ten grains is generally the limit of the daily quantity. Little indeed could be gained by wholly reducing the fever; to reduce it in great part suffices to prevent rapid wasting, to prevent chills and sweats, and to restore appetite; but the local disease itself is not removed. Indirectly, of course, we relieve it, insomuch as the fever and the primary disease act and react upon each other, and the primary failure has more chance of repair if the fever be moderated. But to moderate it seems better than to repress it with a heavy hand, and I have many charts extending over weeks of time in my possession, which show the effect of quinine in bringing the daily oscillations within much narrower parallels, and in thus giving the patient more chance of recovery. Many a case of dangerous pneumonia of the apex and the like has thus ended favourably which looked bad enough until the above means were adopted. Quinine also in doses appropriate to the occasion—and this the thermometer alone can decide—is to be given whenever septic incidents are seen in the course of specific fevers or following them, and if the absorption be slight or temporary, the immediate and repeated use of quinine is invaluable.

Turning now from this free estimate of the value of quinine in septic and hectic states to its effects in specific febrile periods, I speak with less confidence. Sometimes it fails even in very heavy doses to depress the curve at all; sometimes it depresses the curve for a time; but the depression is followed by a bound upwards which the same means cannot control. Such results we generally see in the ascending section of the curve. I have not had much reason to congratulate myself on the use of heavy doses of quinine in depressing the elevation of the first four days of typhus or pneumonia, or in the hyperpyrexia of rheumatic fever; often I have thought my interference to have been positively harmful, but of this it is difficult to be sure. Therefore, when we have to meet immediate danger from a rapid rise, I put no trust in quinine, but resort at once to cold baths, bags of ice, and the like. If, however, a high temperature is doing harm at the latter part of a period, quinine will often act like a charm. If, for example, towards the end of the third week of typhoid, a temperature of 104°·5 or 105° is likely to be too much for the frame already too far consumed to resist it as it would once have done, then 20 grains of quinine is often very valuable; and such doses, repeated as

may be required, may determine the close of the period and release the sufferer. In the second week we get no such success as this. Again, after or during defervescence, we often meet with impulsive elevations of the curve, apparently not due to any septic absorption, but which betray the enfeebled tension of the regulating power of the body; against these quinine is most useful, and often cuts them short at once. Their return may also be prevented by the prescription of five grains of quinine every morning for a few days.—*Pract., Jan.*, 1874, *p.* 32.

4.—ON THE TREATMENT OF TYPHOID FEVER WITH COLD BATHS.

By Professor BEHIER.

[The following paper is from a clinical lecture delivered at the Hôtel-Dieu. The author does not partake of the exclusive enthusiasm which many profess for the treatment of typhoid fever by cold baths in cases attended with unusually high temperature, nor does he recommend its usual and systematic employment.]

The times are gone by in which, according to the old Hippocratic idea, the febrile movement was considered as an effort of nature destined to subdue or to eliminate the morbific principle, a salutary effort which the physician ought rather to favour than to oppose. We now know that this disorderly action is always dangerous and often fatal: dangerous by the exaggeration of internal oxidations and the rapid consumption which accompany the febrile movement: dangerous above all by the modifications of the blood which are produced by these excessive oxidations, which pour into the common medium organic *débris* which are often very abundant, and which are not sufficiently eliminated. But the elevation of temperature is the index and the result, if not the cause (or one of the causes) of this febrile state. We may hope, then, that the intensity and the consequences of the fever will be diminished by means which will reduce the temperature of the body. But if there be a malady in which the efforts of treatment should especially be addressed to the element *fever*, surely it is in the most obstinate and long-continued of our pyrexias—in typhoid fever.

It is about twelve years since I was one of the first to introduce into our country the employment of a valuable therapeutic agent in febrile affections. I speak of *alcohol*. This is not the time or place to speak of the internal action of this remedy. Does it diminish the combustion of the tissue by directly opposing itself to that process, just as, when in a certain degree of concentration, it hinders the continuation of alcoholic fermenta-

tion? I believe this, but it is of little consequence to us just now. An invariable fact is, that in sufficient dose it lowers the central temperature both in the normal and in the febrile states; it is therefore an antipyretic remedy. It is in this character, and not as a tonic, that you see me administer alcohol, in drinkable shape, to our febrile patients, often in considerable doses, under the euphemistic designation of *potion de Todd*. Observe carefully, however, that these doses are divided. Thus, you will prescribe 80 to 200 grammes (3 to 7 ounces) of brandy in 8 ounces of water with a drachm and a half of tincture of cannella and an ounce of syrup. You will give a tablespoonful of this every two hours, diluted with three or four times as much water, if the patient finds the mixture too strong. This remedy, I repeat, gives good results; but I have no hesitation in saying that we are in possession of a much more energetic and habitually efficacious antipyretic remedy in the rational employment of cold water.

The man who restored to honour the treatment of the pyrexias, and especially of typhoid fever, by cold water, is a practitioner of Stettin, Ernest Brand.

If cold water moderates fevers, it is not, perhaps, by moderating combustions (for it is proved, on the contrary, that it tends rather to increase them), but by hastening the departure, the waste, of caloric. Its action is thus diametrically opposed to that of alcohol, quinine, and other agents which directly attack and moderate the febrile combustion; its *rôle* is that of spoliation, not of economy of waste.

In an absolute point of view, hydrotherapy, in whatever form, does not constitute an anti-febrile remedy in the rigorous sense of the word; it does not restrain combustion, it only masks and attenuates to some extent the consequences of combustion, especially the elevation of the temperature of the blood.

Fever does, in fact, betray its action on the economy by two results which are distinct, though closely allied; the rapid consumption of the tissues on the one hand, and the elevation of temperature on the other. Each of these conditions has its own dangers, and consequently affords precise indications. In the slow, chronic fevers (*hectics*, in a word), the danger consists less in the elevation of the temperature than in the gradual and progressive deperishing of the organism; it has therefore never entered the mind of any physician to employ cold water in these cases, and thereby to render more active the very wasting which threatens the patient. On the contrary, that which constitutes the immediate and direct danger in pyrexias when the fever is more acute, so to speak, is not so much the exaggerated histolysis as the very fact of the febrile increase

of heat; the delirium, the convulsions, the drowsiness, the tendency to syncope or asphyxia, the whole *ensemble* of ataxic and adynamic phenomena, is due, we can hardly permit ourselves to doubt, to the pernicious action of a superheated blood upon the nervous centres. In a position such as this, our duty before all things is to cool the blood by the rapid extraction of heat from it; an indication which hydrotherapy alone is capable of fulfilling.

As I have now endeavoured to throw some light for you upon this question, it remains for me to describe to you the mode of treatment which I employed for our patients, and the way in which I believe it is best to arrange the cold baths in the treatment of typhoid fever. Here are the cases:—

Case 1.—A young girl of 19 years, lying in No. 9, Salle St. Antoine. On admission (Nov. 1) she presented all the symptoms of grave typhoid fever. She was then in the sixth day of the disease; the morning temperature was 40°·2 C. (104°·3 F.); that of the day, 40°·6 C. (105°·1 F.) The pulse was 120 to 130. The adynamia was more distinct; the patient was deaf. The gaze was fixed, stupefied, the features sombre and concentrated; she answered questions drily, brusquely, and incoherently. There were *subsultus tendinum* and fibrillary muscular tremors. The tongue and the teeth were foul, the belly moderately tympanitic, gurgling in the iliac fossa. No rose-coloured spots.

In the first days of her stay in our wards she was submitted to the treatment which we habitually apply in cases of this nature (cool spongings, application of many [24] dry cups to the trunk and limbs, *potion de Todd* 100 grammes).

But we did not find any sensible amendment, and on Nov. 8 (fourteenth day of the disease) the weakness was more and more pronounced; she lay on her back, in extreme prostration, with continual sub-delirium. The pulse was small and contracted, the breathing short, hurried, and facial. We observed a marked relative dulness at the bases of both lungs, and abundance of fine sub-crepitant râles (hypostasis).

In presence of such a condition I thought of some more energetic measures; and I prescribed the use of cold baths. On the evening of the 9th the patient was placed in a bath of water at 20° C. (68° F.) She was kept in for about twelve minutes, when she began to shiver; she was then immediately taken out, rubbed briskly with a warm blanket, and replaced in bed.

Next day (the fifteenth) we found the patient in a veritable state of *collapse*. The temperature, which on the previous evening had been 40°·6 C. (105°·1 F.), was now 35°·8 C. (96°·4 F.); the pulse was small, hardly perceptible, 140 a minute. Never-

theless we gave a bath of thirteen minutes' duration. Half an hour after the bath, the patient being warmer again, the axillary temperature was 38°·4 C. instead of 35°·8 (a difference of nearly 5° F.) The pulse was also improved; it was firmer and slower (120).

From the date of this change that marked the beginning of the treatment, the latter was continued without any incident; it consisted of three baths daily of 20° C. (68° F.) temperature, and fifteen minutes' duration. After the 11th of Nov. the axillary temperature never exceeded 38°·2 C. (100°·8 F.); intelligence was completely restored, the sordes on the teeth disappeared, the tongue was moist, with only a thin covering of mucus. The numerous fine sub-crepitant râles that were heard before the treatment were replaced by fewer and coarser sounds; the hypostasis had almost entirely disappeared.

On the 17th we stopped the baths. Temperature normal, tongue moist and clean, appetite fairly good, intelligence perfect, convalescence frankly established. The duration of the disease had been twenty-four days. [Here follows a chart of the pulse and temperature which shows the striking change produced by the baths, but which we are obliged to omit.—ED. *Pract.*]

Case 2.—Marie C., aged 19, No. 25, Salle de Antoine, admitted Nov. 2, 1873. She is a robust nurse, who has lived in Paris for six months, ill for the last four days. Tongue white, with red edges. Diarrhœa, gurgling in the right iliac fossa, numerous rose spots, deafness. Countenance stupid; *subsultus tendinum*, agitated delirium, absolute mutism. The first eight days of her stay in hospital she was treated with simple cold spongings, and *portion de Todd* 100 grammes; the mean temperature was 40° C. (104° F.) in the evening, and 39° C. (102°2 F.) in the morning. On Nov. 8 (eleventh day of disease) there was stiffness of the neck, deglutition was impossible, and there were very distinct cerebro-spinal phenomena. We prescribed cold baths of 20° C. (65°·5 F.), given in the same way as to the former patient, three times in every twenty-four hours. From that day forward the temperature fell and remained always at a moderate level, only once over 39° C. (102°·2 F.). Intelligence and speech returned gradually; on the 20th (twenty-fourth day of the disease) the baths were stopped, the temperature was normal, and we began to feed the patient.

Assuredly we should not be in a position, on the mere authority of two cases, to pronounce definitely on the value of a treatment.

As a formal contra-indication, besides hemorrhage and intestinal perforation, Wunderlich mentions collapse. As to the last, I cannot share this view, and I believe, on the contrary,

that the plunge bath, and cold affusions, are one of the best agents that we possess for dissipating that tendency to paralysis of the vasculo-respiratory centres which is the primary cause of collapse. Our patient in No. 9 is a convincing example of this. On the morning of the fifteenth day she was threatened directly with collapse, the nose and extremities were cold, the axillary temperature was at 96°·8 F., the pulse was thread-like and scarcely to be felt. Half an hour after the cold bath the patient had become warmer again, the pulse and temperature had risen, and all traces of collapse had disappeared.

What are, definitely speaking, the advantages of this treatment, which have gained it so many fervent adepts, and which cause it, in certain countries—Germany for instance—to be employed to the exclusion of almost every other remedy? This is the final point for us to elucidate. A simple glance of the eye, cast upon our thermometric curves, will spare you, so to speak, all commentary. You see that by means of a few baths administered in each twenty-four hours, we succeed in beating down and mastering a febrile movement which everything seemed to show would be violent and lasting. In satisfying this essential indication, the lowering of temperature, hydrotherapy weakens at the same stroke almost the whole group of the symptoms of typhoid pyrexia. The nervous centres are the first and the most happily influenced; the delirium departs, the intellectual torpor is banished, the patient comes back to himself, and interests himself in what surrounds him; at the same time the carphology, the *subsultus tendinum*, and all the other indices of profound nervous perturbation disappear. The respiratory centre also participates in this remarkable modification; the inspirations become deeper, slower, more powerful; hæmatosis is effected more completely, and the bronchi, resuming their contractility, expel the secretion which obstructs them. The skin resumes its tone and its suppleness; the blood propelled by the heart in stronger and more regular waves, circulates more actively, and revives the stagnant secretions. Even the digestive canal presents a notable improvement, the tongue cleans and becomes moist, thirst subsides, and tympany diminishes.

There remains one sole objection—the following: the cold, by forcing the blood towards the internal parts, may favour the production of intestinal hemorrhages.

The statistics of the Leipzig clinic bear out this idea, in appearance at least. In fact, 251 typhoid patients treated with cold water, 18 had intestinal hemorrhage, or 7·1 per cent.; whereas, according to the tables of Louis, Griesinger, Ragaine, &c., with ordinary treatment hemorrhage does not occur in more than 3 to 4 per cent. of the cases.

Wunderlich thinks that the Leipzig statistics are derived from too small a number of patients (251) to be decisive, and believes that we ought to consider the influence of series. He does not lay the blame of the greater frequency of hemorrhage upon the baths, for these bleedings, in the 18 cases in which they were observed were not produced immediately after immersion, but some hours, often half a day, afterwards, when the centripetal repulsion of the blood no longer existed. Finally, which is a fact of the highest importance, these cases of hemorrhage were not serious, and the whole number ended in cure.

According to Wunderlich it would be precisely the employment of the baths which so singularly diminished the gravity of the complications in these cases. The patient, thanks to the treatment, is placed in such conditions that he bears these hemorrhages much better, and they become, so to speak, innocuous. Thus, says he, if intestinal hemorrhages are made more frequent their gravity is much diminished, by cold baths. But before accepting this dictum of Wunderlich, we must remember that I had occasion to point out to you that bleeding from the bowel in typhoid fever has not always the gravity which is usually ascribed to it, and that Graves, Trousseau, and myself had observed many cases in which hemorrhage terminated favourably.—*Practitioner*, *Feb.* 1874, *p.* 81.

5.—ON THE EMPLOYMENT OF STIMULANTS IN THE TREATMENT OF FEVERS.

By Dr. Thomas B. Peacock, Senior Physician to St. Thomas's Hospital.

Some years ago, under the influence of teaching which I cannot but regard as mistaken, stimulants were so largely given in fever and other forms of active disease as to constitute almost the whole treatment, and make it often difficult to decide, when patients were seen, how much of their condition was due to the disease under which they laboured, and how much to the treatment employed. Now, however, a more judicious system is followed, and stimulants are less constantly and freely given. But there seems danger that, in the reaction, the discontinuance of the use of stimulants should be carried too far. Believing, as I do, that the abuse of fermented beverages is the greatest social bane in this country, I should have been glad could I conscientiously have done so, to have recommended the entire abandonment of the use of stimulants in the treatment of fever and other forms of disease. To do so would, however, I believe, be to deprive ourselves of one of the greatest means of alleviation which we possess, though one

which requires great care in its use. It would be very much more satisfactory if the employment of these and other remedies could be reduced to some definite and scientific rules; and endeavours have been made by the careful analysis of the results of different methods of treatment to deduce such general rules. But the cases which we have to treat are so variable in their character that it is impossible so to classify them as to be sure that the cases compared are really similar, and that the results obtained are to be depended upon as exact. I fear, therefore, that we must be content still to leave to the judicious physician the choice of the means of treatment to be pursued in any given case, guided by his knowledge of the disease he has to treat, and his experience of the results of the means employed in other similar cases. So far as the use of stimulants is concerned in the treatment of fever, it is impossible to lay down any general rules; but I believe them to be eminently beneficial when given when the active stage of disease is subsiding, and the patient's power is beginning to give way, with the view of upholding the strength while the disease is in process of cure. If the patient is much prostrated, and the pulse weak, intermittent, or irregular, or abnormally slow, they may confidently be given; and if under their use the patient becomes less restless, the pulse improves in character, and the prostration diminishes, they may safely be persevered with. The amount of stimulus which is required must also be left to the discretion of the practitioner; but 'generally it is more advantageous if exhibited in small doses, repeated when the effect begins to subside. If a given dose exhibited at certain stated intervals seems to excite the patient, it does not follow that no stimulant is required; it may rather be that the dose is too large and the repetition too frequent. When the patient's strength becomes greatly exhausted, as it often does when the crisis of the disease is passing or passed, the very liberal exhibition of stimulants may be necessary; and as the weakness becomes less and more food is taken, the quantity may be gradually lessened; but I think it is a great mistake to suppose that because the patient can be got to take a large quantity of food, the use of stimulant is unnecessary. Food, as I have before said, may easily be given too freely. The best results ensue from the judicious combination of moderate amounts of food and stimulus, and this is especially the case at the period of early convalescence. In estimating also the desirableness of administering stimulants in any given case, the question to be asked is not simply whether the patient will recover without their use, but whether he will make an equally good and rapid recovery without them? Typhus and typhoid are diseases of very variable severity, and this is especially the case with

typhoid, and there are some cases of both forms which will do very well without any stimulant at all. But, on the other hand, I have not unfrequently seen, in cases of fever which have never been very severe, that the patient will go on from day to day without making any material progress so long as stimulants are withheld, while on the exhibition of a small amount of wine or spirit there has been a marked and quite unmistakeable improvement in the condition of the patient, and the case has afterwards steadily progressed to recovery. What I would wish to impress upon you is, not that you should make up your minds to give or not to give stimulants in cases of fever, but that you should hold yourselves free to have recourse to them, or not to use them, according to the apparent requirements of the several cases which you have to treat. There can be no worse fault in medical practice than to adopt a hard-and-fast rule as to the plans of treatment or special remedies to be employed in any form of disease. You should endeavour to make yourselves fully acquainted with the nature of the disease you have to treat and the condition of your patient, and then prescribe the course of treatment which you think will most conduce to his recovery.—*Lancet, March* 28, 1874, *p.* 437.

6.—ON PYÆMIA.

By the EDITOR OF THE LANCET.

A clear, logical, and scientific definition of the disease known as Pyæmia is, we fear, a desideratum not easily attainable; yet, if we employ the term at all, it is of the highest importance that a distinct and definite meaning should be attached to it, so that the same disease should be always indicated by the same term. If different persons attach different meanings to the same word, it is evident that it is impossible to engage in any profitable discussion until all the misconceptions by which the definition has hitherto been encumbered have been cleared away. If this had been kept more prominently in view at the recent discussion at the Clinical Society it is highly probable that, instead of having now to attempt to reconcile things apparently contradictory, we should have been engaged in registering some of the fixed and settled clinical and pathological facts concerning pyæmia. Many authorities object entirely to the word "pyæmia," and deny the existence of such a pathological condition. Taken in its strict etymological sense, the word is undoubtedly a misnomer; but provided that this fact is always recognised, and that only certain symptoms and lesions, and no other, are comprehended by the term, it is of small importance whether we designate the disease by this name or by that.

With this restriction, therefore, pyæmia may be defined as a diseased state of the general system, in which high temperature, accompanied by severe rigors alternating with profuse sweatings, supervene on an inflammatory or suppurative process. The blood, which is poisoned and unfit to perform its normal functions, contains an excess of white corpuscles and of fibrin. There are, moreover, nervous symptoms of a low type; and distributed over various parts of the body may be found diffuse and metastatic inflammations and suppuration, which after death are recognised as hemorrhages, infarcts, consolidations, softenings, and suppurations in the joints, serious cavities, and internal viscera. The veins adjoining the suppurating tissues usually contain clots in a state of disintegration, or softened at the centre into puriform fluids. The disease, as thus described, is well known, and for the most part easily recognised. There are, however, cases which sometimes show themselves in which there is evidence of intense blood-poisoning, to which the patient may rapidly succumb, or from which he may ultimately recover without the occurrence of diffuse or secondary inflammation. This condition, which has been called "septicæmia," is by some regarded as only a stage of pyæmia, but by others it is thought to be a definite and distinct disease, which, although supervening on inflammatory processes, is dependent on a cause altogether different from that of pyæmia. There is also another kind of general reaction following wounds, which has been designated "traumatic fever." In the latter there is a high degree of temperature, probably due to the absorption of some of the early inflammatory products or blood which have undergone degenerative changes. There are therefore three different conditions which may follow the receipt of an injury, and occur during the inflammatory or suppurative processes. These have been named by Billroth traumatic and inflammatory fever, septicæmia, and suppurative fever or pyæmia. This classification, it will be seen, corresponds very closely with that proposed by Mr. Callender in his remarks at the Clinical Society, in which he described three forms of blood-poisoning supervening on wounds—viz., primary septicæmia due to the absorption of the acrid and irritating fluids thrown out into the wound within the first twenty-four or thirty-six hours; secondary septicæmia, the result of absorption of products arising from the decomposition of residua, whether those residua be sloughs or matter passing into a putrid state; and, lastly, thrombolosis, which appears to include the ordinary typical cases of pyæmia, in which there is plugging in the veins, disintegration of the clot, and dissemination of the poisoned, broken-up fragments, giving rise to the secondary deposits in the internal viscera. Now, although some cases may present themselves in which it

is difficult to determine whether they should be placed under this or that category, it is generally sufficiently easy in practice to determine the nature of any individual case. Seeing, then, that tolerably well defined lines of distinction have been marked out and widely accepted, we are opposed to any unnecessary transgression of these limits. It must be observed, however, that many excellent authorities, chief among whom is Mr. Savory, deny that there is any essential distinction between septicæmia and pyæmia so called—that septicæmia is but an early stage of pyæmia due to the absorption of a large quantity or a powerful dose of the morbific material, which destroys the patient before the secondary changes have had time to occur. This is doubtless true in some cases, but not in all; for there is a form of septicæmia, which is not unfrequently recovered from, which may be accidental or experimental, from the absorption of putrid matters or some of the products of decomposition. Frequently in these cases no secondary or metastatic changes take place. It may therefore reasonably be conceived that, while a condition of septicæmia may be merely the early stage of what would become in time a genuine pyæmia, there is another form of septicæmia in no way or only remotely connected with the disease commonly known as pyæmia.

It is no doubt true that in every case of pyæmia there may be distinguished two separate processes—the toxic and the mechanical; the latter of which leads to the formation of secondary deposits of various kinds, the ulterior changes in which will greatly depend on their nature and quality. These two processes are, however, intimately associated and practically inseparable, neither separately being efficient to produce all the phenomena of genuine pyæmia. The true nature of the toxic matter which gives rise to the blood-poisoning in pyæmia, its origin and the mode of its operation, are not yet fully known. These are, indeed, the *questiones vexatæ* of pyæmia; for the mechanical processes are capable of a ready explanation, provided that we knew what it is that produces the profound alterations in the character of the blood, and which spoils it for the performance of its proper functions. The fact that pyæmia is generally preceded by the suppurative process, and especially when that process has its seat in bone or tissues freely supplied with veins, has naturally led to the opinion that the poison is in some way or another connected with suppuration; and it has, indeed, been affirmed that the morbific material arises from the decomposition of pus, and that this material is then absorbed into the venous system, leading to disintegration of the thrombi which form in veins, and then poisoning in some manner the general mass of the blood. Chemists, as Bergmann, Verneuil, and Dr. B. W. Richardson,

have even isolated a principle, sepsin or septin, which they assert is capable of inducing pyæmia. This principle is described as an alkaloid which is generated by the decomposition of albuminoid matter. But against the specificity of this body it is maintained that septicæmia and metastatic inflammation and suppuration may follow the absorption of some of the other products of decomposition.

The opinion that the *materies morbi* is something which is primarily developed in decomposing pus, and that this morbific element is directly absorbed into the general system by the veins or lymphatics, derives its chief support from the numerous and careful experiments of Virchow. But this pathologist attempted to show that the pus-corpuscles were themselves altogether negative in their action; and in proof of this he asserted that pus-corpuscles as such could not pass into the venous system without producing symptoms altogether different from those of pyæmia, and he showed that, as far as the lymphatic vessels were concerned, it is impossible for a particle the size of a pus corpuscle to pass through the fine ramifications of a lymphatic gland, so that it could not be absorbed by this channel. Virchow therefore explains the phenomena of pyæmia in the following manner: Unhealthy suppuration is set up in a wound, and the ichorous fluid which results from the decomposition of the pus, penetrating into the thrombi which have formed in the adjacent veins, causes them to disintegrate and become disseminated through the capillary system, giving rise to the changes known as secondary deposits. But although this view affords an intelligible and clear explanation of many of the effects of pyæmia, some of the secondary lesions are left unaccounted for. There are, for example, lesions occurring in the second set of capillaries beyond the original wound, while the first set of capillaries altogether escape. Hence a great difficulty in the way of the universal adoption of the belief in the embolic origin of all secondary lesions presents itself. It has, however, since been shown by O. Weber and others that some particles, especially floccules of pus, may pass one set of capillaries, but become entangled in another set. But even this explanation will not account for many other secondary inflammations, especially those of the pleura, skin, and joints.

On the other hand, some have alleged that the poison is a miasm which possesses no specific properties, but which exists whenever the atmosphere becomes unduly charged with organic matters, and which may attack persons through the medium of wounds, the mouth, the lungs, or intestines. This miasm, which consists of dry particles of organic matter, pus-cells, epidermic cells, and perhaps germs, is stated to float in the air, and to be thus carried from patient to patient. When these

particles come in contact with a wound, they inaugurate certain changes in the wound, which result in the production of poisonous material, which is absorbed into the general system by the veins and lymphatics. Many observers, indeed, have shown that such particles do actually exist in the air of hospital wards, and they further maintain that these may be the bearers of poisons, which may be absorbed by any opened wound. It follows, therefore, that, according to believers in this doctrine, the ventilation of wards and the constant purification of air becomes an all-important element in the prevention of pyæmia.

In favour of the view that the toxic matter is developed in decomposing pus is the frequent supervention of pyæmia on suppurative processes in localities favourable for the retention of pus and its subsequent decomposition, and the ready absorption of the morbific elements by the adjacent veins. One of the most frequent antecedents of pyæmia is osteo-myelitis. In this affection the pus accumulates in the interstices of the bone and the small venules, which on account of their intimate connexion with the osseous canals, are unable to contract, are constantly bathed with pus, and are thus, as it were, always ready to receive the poisonous materials which may be developed by decomposition of pus. When once the morbific matter has gained an entrance into the osseous veins it is readily transmitted to the systemic veins by means of the free communication which Demarquay and others have shown to exist between the two sets of vessels.

But Dr. Burdon-Sanderson maintains that the morbific material of contagious disease in general consists, not of substances chemically dissolved in infective liquids, but of extremely minute particles suspended in it—of organisms, in fact, which he calls microzymes. In the experiments the results of which were recently published in the Pathological Society's Transactions, he asserted that in all infective inflammations in the lower animals microzymes abound in the exudation liquids; and that the same forms are also to be found in the blood of animals when in a state of acute infective fever. He further asserted that the intensity of the action of the infective fluid is intimately related to the number and the activity of the microzymes which it contained. In very infective fluids the extremely minute single rods of the Bacterium vibrio were found; while in the fluids of more protracted inflammations, and particularly in the contents of infective abscess which had existed for comparatively long periods, the prevailing forms of microzymes were spheroids, dumb-bells, and chains of the Bacterium varicosum.

Although Dr. Sanderson did not actually state that these organisms are the cause of pyæmic infection, or that they con-

stitute the pyæmia contagion, he declared that there can be no doubt that by their presence in exudative liquid, however that presence is to be accounted for, they modify in a very decided manner both the local and constitutional affections. But to this it has been objected that, although microzymes do exist in the infective fluids of pyæmia, their presence does not afford any explanation of the nature of the poison and the mode in which it acts. Microzymes exist in abundance in other fluids, and have even been regarded as the exciting causes of tetanus and many other diseases, and cannot therefore be deemed to be the real and essential cause of pyæmic infection.

Having considered the nature of the *materies morbi* of pyæmia and the mode of its development, we shall now briefly inquire into the means by which it gains entrance into the general system and produces its characteristic effects. As already indicated, the usual, if not constant, place of entrance is the surface of a wound. But all wounds are not equally favourable to the admission of the morbific element. Some wounds, even of large size, are comparatively harmless; while others, although small, are exceedingly prone to become the seat of abnormal changes and the centre from which the poison spreads. Experimental and clinical experience show that there are at least three channels by which the poison may be absorbed—namely, the veins, the lymphatics, and the cellular tissues. That the veins are most frequently the channels by which the poison is admitted into the blood appears to have been satisfactorily proved; but the evidence of absorption by lymphatics and the cellular tissue is less clear and certain. From the earliest times the veins have been regarded as playing an important part in the production of pyæmia. In fact by some, even at this day, they are held to be the *fons et origo mali*. Formerly, it was thought that a suppurative inflammation was set up in the veins adjoining a wound; and that the pus which was thus formed entered the general circulation, and in that manner produced not only the toxic but also the mechanical effects of the disease. But careful recent researches have shown the fallacy of this explanation. Now we know that pyæmia does not really mean pus in the blood, that suppurative phlebitis probably never exists, and that even the pus or pus-like cells which are found in the thrombi of veins are not the essential poisoners of the blood, but are accidental phenomena, the probable result of the migration of white blood-corpuscles. It would almost appear that the veins take a comparatively small part in the production of pyæmia. It is true that the experiments and observations of Virchow have shown that the poisoned disintegrated thrombi which form in the veins may be not only the carriers of the poison, but may, by becoming

arrested in some portion of the capillary system, give rise to lesions known as secondary deposits and inflammations. But there is nothing peculiar in the formation of thrombi in the veins of a patient suffering with pyæmia. Thrombi are necessary to normal repair; but in pyæmia, instead of undergoing the ordinary changes of healthy repair, they generally undergo softening and disintegration. But they do not always undergo this disintegration, for after death the most careful examination may fail to detect any disintegration of the clot. This fact affords one of the most striking arguments against the embolic origin of all secondary changes. It may be that, owing to the hyperinotic condition of the blood, small fragments of fibrin are constantly being deposited on the centripetal end of the thrombus, and are swept away again as soon as they reach the strong current of the collateral circulation. But even this explanation fails to account for the exudations and inflammations of tissues, not only topographically, but anatomically remote from the seat of the vein which supplies the thrombic fragments. A satisfactory solution of the difficulty may, we think, be found in the changes which exist in the blood, and by regarding the lymphatics and cellular tissues as sources of absorption of the poison. The blood not only contains an excess of white corpuscles and of fibrin, but it is altered in its physiological properties, either by containing suspended in it poisoned particles and poisoned fluids, or by the general changes which result from the fermentation set up catalytically by the presence of the morbific elements. In such a state of the blood there exist, therefore, certain conditions which are favourable to the development of secondary inflammations altogether apart from the influence of embolia. The white corpuscles of the blood which crowd along the walls of the capillaries may, under the altered relations between blood and tissue, become blocked in any portion of the capillary system; and being contaminated with poisonous material, they set up irritative changes, which result in extravasations, exudations, inflammations, and suppurations. These changes occur more frequently in some parts than in others; but what it is that determines the special disposition of certain tissues to react to altered blood conditions while others escape, we cannot say, but may describe it as a vulnerability of tissue. This vulnerability of certain tissues to certain poisons is exemplified in many diseases: in measles and small-pox, for instance, it is the skin; in rheumatism, it is the fibrous tissues and the joints; in typhoid fever, it is Peyer's glands, and so on; and in pyæmia it is not improbable that the serous membranes may be the points of election for the reaction to the morbific matter. It is, moreover, interesting to note that the secondary inflam-

mations affect chiefly parts of great functional activity, and organs which in normal physiological conditions of nutrition aid in the elaboration and in the depuration of the blood. From this it would appear that in the morbid condition of the blood and tissues the ordinary processes of nutrition are perverted, and that irritative and inflammatory reaction results. Other causes of secondary inflammations may exist in the irritation which the poisoned matters may produce in the lymphatics. It may be true as Virchow has shown, that pus-cells cannot pass through lymphatic glands; but both the cells and the fluid of pus may get into the lymphatic vessels, and set up irritation and inflammation, either at the valves in the vessel or at the glands, so that inflammation and abscess may really arise in the course of lymphatic vessels at a considerable distance from any known gland. Lastly, the amœbiform movements and migratory character of living pus-cells and ordinary white blood-cells render probable the hypothesis that secondary inflammations and abscesses may have their origin from cells which have wandered along the cellular planes of the body to a distance comparatively remote from the seat of the wound. By either or both of these means secondary inflammations may arise which cannot be explained by the dissemination of poisoned emboli and their arrest in certain parts of the capillary system.

We have now considered some of the chief pathological facts of pyæmia, the essential nature of the disease, its mode of origin, and its chief mechanical and anatomical manifestations. We have purposely refrained from discussing the subtle questions concerning the changes which are imposed upon the blood by the action of the morbific material, as such a course would have led us too far into the domain of speculative pathology, our present object being merely to supplement the remarks which we made during the progress of the recent discussion on the indications for the prevention of pyæmia in hospital and private practice. We may thus sum up our knowledge:—The poisonous material of pyæmia, whatever be its real nature, is closely related to the suppurative process, and may actually originate in certain unhealthy conditions of that process. But even where the poison does not really arise in a suppurating wound, a certain disposition of the tissues of the wound is necessary to admit of its entrance into the general system. There, are, for instance, strong facts to prove that a perfectly healthy state of a wound is to a great extent inhibitory to the absorption of the poison. Billroth has accounted for this on the ground that perfectly healthy granulations prevent the intromission of organic particles and fluids; and in the same way may be explained the cases where no evil effects result

from the presence of decomposing fluids in wounds with healthy granulations. Whether this poison is capable of being transferred from one wound to another through the medium of the air may be doubtful, but there is strong evidence that the virus may be conveyed to wounds and implanted there by means of sponges, dressings, and instruments. The outcome of all this is, that, if pyæmia is to be prevented, the well-established rules of surgery and hygiene must be persistently employed in the dressing of wounds and the treatment of patients.—*Lancet, March* 28, *and April* 4, 1874, *pp.* 446, 483.

7.—ON PYÆMIA.

By Dr. J. HUGHES BENNETT, Professor of Physiology and Clinical Medicine in the University of Edinburgh.

I have been greatly interested in the discussion which has taken place on pyæmia in the Clinical Society of London. Nothing could be more reasonable than the suggestion of Mr. Hulke, that there should be a clear understanding as to what the Society were talking about. "It appears to me," he is reported to have said, "that Dr. Bastian's remarks are most pertinent on the question of septicæmia, but I do not see their bearing on the subject of pyæmia." In truth, in this, as in many other discussions which take place in medical societies, acurate definitions and logical precision of language have been too frequently forgotten. A notable example was presented last year in a discussion which took place in the Pathological Society of London, on the subject of tubercle. In no sense whatever could it be said to have advanced our knowledge, all the speakers apparently having puzzled themselves and bewildered one another, in a vain effort to explain German mysticisms, and apply new terms to very old and well known morbid conditions. The confusion which prevailed on the subject of pyæmia may, I think, be dissipated; and I propose, in this short communication, to make an effort to do so.

The term pyæmia was introduced by Piorry, and literally means pus in the blood, or purulent blood. He supposed that the formidable symptoms, which constitute the dread of surgeons, namely, the rigor, typhoid fever, depression, and multiple abscesses, generally terminating in death, following operations or injuries, were owing to pus in the blood. However the pus was introduced, whether by absorption from wounds or from phlebitis, or from an imaginary lesion he called hæmitis, or inflammation of the blood itself, the result was purulent blood, or pyæmia, and the cause of the fatal symp-

toms. But M. Piorry was no histologist. Had he examined the blood with a microscope, he would have found no pus-cells there; for, in fact, the blood of pyæmia contains no pus. Our present knowledge, therefore, demonstrates that the disease called by practical surgeons and obstetricians pyæmia is not necessarily caused by pus in the blood. This is further shown by the fact that if, as Mr. Hulke truly stated, we inject recent or so-called laudable pus into the blood of an animal, it produces no injurious effects. I have frequently performed this experiment (Clinical Lectures, 5th ed., p. 906), as have Sédillot and other pathologists. But, if putrid or ichorous pus be so injected, then we have the symptoms of so-called pyæmia; and the same result occurs if we inject putrid fluids, irrespective of purulent formations. It follows, therefore, that it is not pus, as pus, which produces the mischief, but any organic fluid in a state of putrefaction, that causes the blood-lesion, and, as a result, the group of symptoms now called those of pyæmia. That pus is easily decomposed has been long known; and to the absorption of that fluid, when putrid, the French surgeons Boyer and Bonnet, many years ago, attributed the noxious effects referred to as pyæmia. This condition was well-named by Vogel ichorrhæmia, which is one of the varieties of septicæmia.

It appears to me, therefore, that Mr. Savory expressed the correct view on this subject, but did not appear to see that the term pyæmia ought to be abolished from medicine and surgery, and that the form of septicæmia to which he referred is correctly expressed by the term ichorrhæmia. In what the nature of septicæmia consists, is unknown. Why one variety should produce the phenomena of small-pox, another of measles, and a third of ichorrhæmia, we are ignorant. It is the same ultimate fact in pathology that exists in therapeutics, in reference to the proportion of different poisons or drugs. All we can do is to guard against the morbid influences, or modify them when present. It is in this point of view that the observations of Dr. Bastian are of direct practical importance. All trustworthy research indicates that the poisonous properties are not attached to imaginary germs constantly floating in the atmosphere, but rather to the ultimate molecules of organic matter, animal or vegetable, which belong to or enter the economy. I believe these not to be hypothetical, but visible molecules, detectable by good microscopes, and that, in the case of ichorrhæmia, the histological examination of the granulations of, and the discharges from, suppurating surfaces will do much to indicate how and when they are to be removed or destroyed.

Although I have endeavourad to show that, in the morbid condition called pyæmia by practical surgeons, there is no purulent blood, and that the expression is wholly inapplicable,

there can be no question there is a true pyæmia, or suppuration of the blood. This I described so long ago as 1845 (Edinburgh Medical and Surgical Journal, October, 1845), in a case where I found, in a case of hypertrophied spleen and liver, that the blood was crowded with pus-cells. But in that case, I took great pains to show that there was no inflammation anywhere in the economy—no suppurating sore, no phlebitis. Nevertheless, as I had no doubt that pus existed in the blood, I called the case one of suppuration of that fluid without inflammation. But the pathologists of that day could not comprehend the existence of suppuration without inflammation, and hence I subsequently called the morbid condition I discovered leucocythæmia, or white-cell blood, because that term "expresses the simple facts, or a pathological state, and involves no theory." By some, it was supposed that the colourless corpuscles of the blood were different from pus-cells, and that they rendered the blood white, producing the white blood of Hippocates. This error still prevails among the German pathologists, who call this morbid condition leukhæmia. It is now, however, recognised that pus-cells, lymph-cells, young mucus and epithelial cells, and the colourless cells of the blood are identical, and hence Robin has applied to the whole group the name of leucocytes or white cells. To have retained the term suppuration of the blood, or true pyæmia, in face of the constant employment of this last term by surgeons, to express a fatal poisoning of the blood, could only have led to confusion; although, as a matter of fact, leucocythæmia is the only purulent blood known in pathology, and the only suppuration of that fluid with which we are acquainted.

I propose, therefore, in order to avoid the confusion which now prevails from the incorrect employment of terms that can only mislead, to suggest the following definitions.

Septicæmia.—Poisoning of the blood from every source—a general term.

Ichorrhæmia.—Poisoning of the blood from ichorous matter or decomposed pus.

Leucocythæmia.—White cell-blood; excess of colourless cell in the blood.

Leukæmia.—White blood, from excess of fatty matter in the blood, so-called chylous or milky blood.

Pycemia.—This term has no meaning, unless applied to ichorrhæmia or leucocythæmia. Its employment can only lead to confusion.—*Brit. Med. Journal, March* 14, 1874, *p* 339.

8.—DR. HUGHES BENNETT'S STRICTURES ON THE "TUBERCLE" AND "PYÆMIA" DISCUSSIONS.

By Dr. LIONEL S. BEALE, F.R.S.

"In truth, in this, as in many other discussions which take place in medical societies, accurate definitions and logical precision of language have been too frequently forgotten." Such are the words in which Dr. Bennett criticises our discussions last year on the subject of tubercle. He speaks of the speakers having "bewildered one another" and "puzzled themselves," but tells us that he thinks the confusion which prevailed upon the subject of pyæmia "may be dissipated."

Dr. Bennett tells us there is a "true pyæmia or suppuration of the blood;" and that, in a case of "hypertrophied spleen and liver," the blood was crowded with "pus-cells." As "pus existed in the blood," the case, he declares, was one of "suppuration of that fluid without inflammation." It is now, Dr. Bennett affirms, "*recognised that pus-cells, lymph-cells, young mucus and epithelial cells, and the colourless cells of the blood, are identical.*"

I know not what other pathologists may think of Dr. Bennett's effort to dissipate the confusion which exists; but I cannot allow his paper to pass without expressing my dissent from every one of the above propositions. Dr. Bennett uses the word "pus-cell," as it seems to me, most improperly. He has not proved that the bodies he calls pus-cells are of that nature. But to assert that pus-cells and young mucus and epithelial cells are *identical*, is certainly open to grave objection. That they are like one another to look at, that they may exhibit similar reactions when tested, that they may be alike in chemical composition, may be true; but surely things may be like one another in many particulars without being *identical*. Although the bodies referred to by Dr. Bennett are alike in many points, no two are alike in all, and each has properties peculiar to it; each has a very different life-history, and performs a different office. They may be called by the same name, but differences cannot be dissipated in that way. As Dr. Bennett says, they have been *called* leucocytes or "white cells;" but they are not *cells*, nor are they *white*.

Dr. Bennett is surely as much open to the charge of forgetting "accurate definitions and logical precision of language" as any of those who joined in the discussion of tubercle last year. Dr. Bennett would introduce terrible confusion by calling leucocythæmia "suppuration of the blood." For my part, I cannot imagine any phrase open to more serious objections than this; and, whatever may be the fate of *pyæmia*, I do hope neither

the Pathological Society of London nor any other Society will consider *pyœmia, suppuration of the blood, and leucocythœmia* identical.—*British Med. Journal, March* 21, 1874, *p.* 1874.

9.—ON CANCER.

By CAMPBELL DE MORGAN, Esq., F.R.S., Surgeon to the Middlesex Hospital.

Why should malignant growths, some cancers especially, when once formed, infect neighbouring and distant parts, and ultimately contaminate the system? The knowledge we have of late years acquired of the independent actions of cells renders a solution more easy, though I admit that it is not free from difficulties. It has long been known that cells are dispersed around a cancer tumour, the direction and rapidity of the dispersion varying according to the density of the tissue, and the abundance of connective-tissue spaces or of lymphatic or vascular networks in which it lies. It is now known that the white blood-corpuscles can wander from their vessels; more recently it has been seen that cancer-cells—like the white corpuscles—show amœboid movements, and can thus travel independently in tissue spaces, or even permeate delicate membrane. There is no doubt, I suppose, that these cells, whether the matter that forms their nuclei, or the free granules, are the active agents in the reproduction of cancer. We can understand that these cancer-germs may thus travel through their own power, or be carried along in the vascular systems or connective spaces in every possible direction. And it must be remembered that cancer is for the most part a structure infiltrating itself among the tissues in which it lies—not surrounded by a capsule or limitary membrane of any sort, as are so many non-infecting tumours. The newly developing and active cells are always in direct contact with the healthy tissues, on which, in fact, they are living. Once set free, the cells may travel in the three channels I have indicated. First, and chiefly, they may travel along the lymphatics to the glands, where they will form new centres of growth, and by continual invasion affect a series of them, and perhaps contaminate the lymph itself. Or, secondly, they may travel through the surrounding tissues, their direction being often determined by the density of the tissue. Thus it is often noticed that after breast operations the disease will reappear above or below the cicatrix, and will travel on the same plane, not passing across the dense cicatricial tissue. The disease will seem to prefer the easier course, but the densest tissues will not prevent this permeation. In malignant intra-ocular growths the germs are found at times extending through

the dense sclerotic and then bursting out into a new growth in the free periocular structures. Thirdly, they may pass into the bloodvessels, and thus into the general current of the circulation; hence, we may get infection of internal organs, or of parts widely removed from the original tumour. We have evidence of this mode of conveyance in many recorded cases of sarcoma and enchondroma, and the secondary cancer growths in the lung and liver, so like in many respects to the deposits we get in pyæmia, point to the same mode of origin. Besides these principal modes of conveyance, we meet at times with evidence of conveyance in free cavities by gravitation, and sometimes, but rarely, of implantation by auto-inoculation.

There are some tumours of which the elements are disseminated far more rapidly than are those of cancer; on the other hand, some cancers show little tendency to dissemination. A small melanotic sarcoma, for example, will often give rise to numberless secondary growths in a marvellously short space of time. This is perhaps due to the great activity of growth and the abundance of granular matter which often characterise this form of tumour. If we look to epithelioma, on the other hand, we see that, great and destructive as may be its local action, yet its area is usually limited to the first part invaded and the contiguous lymphatic glands. The structure explains this. The component cells are coarser, have less independent movement, are more packed together, sometimes becoming coherent. They may pass into the lymphatics—coarser matter will do that,—but, like the cells from which they originate, they are little disposed to wander; the tumour grows more by continuity, less by dispersion. This is still more marked in rodent cancer, where the cell activity is at a minimum and the ulcerative keeps pace with the formative processes.

It seems to me, then, that we can account for the diffusion of cancer, when once formed, without calling to our aid a pre-existing or concurrent disease of the blood.

Admitting that cancer, like other infecting tumours, is reproduced by the growth of germs which have been conveyed from it to distant parts, the question arises, How can we account for the fact that after removal there may be no reproduction for years? Can it be imagined that during all this interval the germs of disease deposited from the original tumour have remained inactive? Is not this contrary to the nature of cancer, and does it not show that there is a cancerous poison in the system? I admit the difficulty; but it is not removed by assuming that the blood or the system is primarily diseased. My conviction is that cancer and other tumours may remain in a rudimentary state for an indefinite length of time. In the case of recurrent cancer we often see that an enlarged and hard

gland which is left after the removal of the main tumour will remain quiescent for years, and that then active growth will set in. The same will take place in a cicatrix which has perhaps remained hard. It may be said that this is only an evidence of the return of the blood to a cancerous state. But it is as difficult to understand the quiescence of the blood during this long time as of the cancer germs. Many facts, both in normal and morbid development, are indicative of the possibility of tissue remaining unchanged for long periods, and then taking on new phases of growth. Such are, for example, the change of epiphysial cartilage to bone after twenty years, the change in the female breast at the period of puberty, the shedding of the milk-teeth and the development of the second set. Still more to the point are, perhaps, such instances as the frequent enlargement of the prostate gland in advanced age, or the growth of hair on the female face from follicles which had existed, but in an inactive state, for sixty years. It is not at all improbable that what is here seen as a result of advancing degeneration often plays its part in cancer development. It is, probable, too, though it may not admit of proof, that in those cases, which are so common, of hereditary lipoma or sebaceous or dermal cyst, the rudiments are present from the first. We know as a fact that such tumours will remain small, only just perceptible perhaps for twenty or thirty years, and will then take on active growth. In the imperfect structure of an inefficient mammary gland, or an abortive testicle, is there not present from the earliest period some form of indifferent tissue prone to irregular development, but quiescent during the active periods of life? This is rendered the more probable from what is so frequently found in connection with epithelioma. We have here on the surface what may be hidden in the case of the deeper glands. Scales, or warts, or moles, and some imperfect epithelial growths, may remain without change for fifty years, and then may become the seats of a cancerous growth. If such a scale or other imperfect tissue were persistently irritated, it would doubtless become cancerous at an earlier period. Cancer has been there potentially for years; but its time has not come. Such, I believe, is the explanation of the fact that cancer germs which have wandered from the parent tumour may remain quiet for indefinite lengths of time. I can see no other way of accounting for such a case as this. A tumour is removed; a year afterwards a small nodule, not so large as a split pea, is discovered and removed, no other nodule being perceptible after careful search. After another long interval, another small nodule is found and removed; and again there is quiescence. This may be again repeated. Can we suppose that there are continual reproductions of a blood poison followed by periods of healthy condition?

While I believe it probable that the germs of cancer may thus remain in a sort of dormant condition for long periods of time, I would by no means imply that there is not in cancerous patients a special disposition to tissue change located in some, but not in all the structures of the body. The general tendency to degeneration may give rise to the primary local change; so may local irritation, Thus, out of a hundred chimney-sweeps or clay-pipe smokers, a certain number may have chimney-sweep cancer or lip cancer, the number varying perhaps according to the duration and extent of the irritation. But the majority will not become cancerous, irritate how you will; and of the remainder, few probably would have cancer, unless irritation were applied. The same remark may be made as to such ordinary growths as warts. In some the least irritation will produce them; in others they never will appear. Like other tumours, too, they have their seats of election, and their favourable period of life, and they often appear to be inherited.

There are other facts observable in the later course of a cancerous disease which are difficult of explanation either on the hypothesis of blood disease or of localisation. For example, a local cancer in an ordinary situation may give rise to a general disease of a structure totally different from it. We meet, for example, with a wide-spread disease of the osseous system following a breast cancer, and the disease may not show itself as cancer deposit, but as entire absorption. This may be seen in many of the bodies of the vertebræ, which may totally or partially disappear without any cancer growth being detected, and while yet cancer deposit is taking place in other bones. These, no doubt, are exceptional cases. We find, however, that natural tissues are at times disposed to undergo changes which are equally inexplicable. The changes taking place in some out of all the muscles of the body, the fibrous growths on a number of the nerve-cords in multiple neuroma, and other cases, show that local tendencies to tissue-change exist in detached parts of general tissues. They may be allied to the degenerative changes which pervade unequally particular tracts of the arterial system. We cannot as yet account for these and such-like facts; but whatever may be the local condition which thus determines special tissue-change, whether its seat be in the parenchyma or in the nervous system, the same may possibly determine a preference for deposit or degeneration under the conditions of progressive cancer. We have, in fact, to solve the question, Why have diseases seats of election at all?

Another remarkable and not very explicable phenomenon is the arrest of cancer growth and the gradual wasting of the diseased mass. This is an occasional event which is very im-

portant, as it encourages us to hope that a cure may yet be found for the disease. Although this recession of cancer is sometimes coincident with the development of tubercle, I doubt whether we have sufficient data on which to establish the conclusion that the one disease is antagonistic to the other. It is, however, worthy of note that, frequent as is the occurrence of cancer in consumptive families, the co-existence of the two diseases in one member is not a common event. But cancer is a disease of the middle and later periods of life, and tubercle generally of an earlier one. Those who live to be victims of cancer have passed the period when tubercle would be most likely to set in. That this arrest or recession is not due to any exhaustion of the cancerous element in the body is shown by the fact that, while the disease is retrogressive in one part, it is in active growth in another. The retrogression differs from the degeneration of the earlier formed cells, which is constantly going on in the interior of tumours, as shown by the presence of fat-granules, compound granular cells, and such like. In the case alluded to, the activity of the whole mass is arrested, new cells cease to be formed, and the tumour fades. I doubt whether this can be from the plugging of the vessels feeding the tumour. This no doubt often takes place, and the death of some portion of the mass may be the result. But when we find a widely spread mass becoming quiescent and then fading throughout its whole extent, we must, I think, look to some other cause—for cancer is so far like a parasite that it draws its nourishment largely from the tissues in which it lies. When, for example, a nodule of enchondroma or of sarcoma is detached and floated off in the current of the circulation, and afterwards grows in the lung or the liver, it must in the first instance obtain its nourishment from surrounding tissues. We must look elsewhere for an explanation. It only shifts the difficulty back a stage or two, as I have so often done before, to suggest that the recession of cancer takes place in obedience to the law under which local atrophy, independent of inflammation or disuse, may occur; or that it may be due to some want of organising power inherent in it from the first, as some cancers seem born to be atrophic. It is, under any circumstances, a most important subject for investigation.

Lastly, I would suggest the question—Why is it that cancer, if a blood disease, should be so pre-eminently a disease of women? Taking all cases together, I suppose we should not be far wrong in saying that the uterus and the mammary gland are the seats of the disease in 90 per cent. Does it not look as if the mere tissue-changes dependent on the peculiar vital conditions of these organs were the starting-points of the disease? Were it not so, and if the disease were from the first in the

blood, surely we should find that men were as prone to cancer as women. Other forms of tumour are nearly as prevalent in the one sex as the other. It is a point worthy of consideration. —*Medical Times and Gazette, March* 7, 1874, *p.* 259.

10.—ON CANCER OF THE BREAST.

By CHRISTOPHER HEATH, Esq., Surgeon to University College Hospital.

Sir Astley Cooper gave as the result of his vast experience two years for the full development of cancer in the breast, and from six months to two years more as the limit of the patient's life; but Mr. Sibley's statistics of the cancer-wards of the Middlesex Hospital give fifty-three months as the average of life when the primary disease is removed, but only thirty-two months when the case is left to nature. This is taking all cases of cancer together; but Mr. Baker's statistics are still more useful, since they separate scirrhus from medullary disease, with the following results:—In scirrhus, when left alone, the average duration of life is forty-three months, and when operated on, fifty-five months; in medullary disease, when left alone, life averages about twenty months, and when operated on, forty-four months. You will see, therefore, how poor a chance our second patient had if left untreated; and my own experience has fully convinced me that to leave a rapidly growing cancer of the softer variety to burst through the skin and form a large fungating mass beneath the patient's face is great cruelty, when it is still possible, by even a severe operation, to give some months of comparative comfort, and to render the disease, when it returns, much less offensive and trying to the patient, by ridding her of the bulky breast.

To do good, however, in any case of cancer, the treatment must be very thorough; for any treatment, whether by the knife or caustic, which does not remove the whole of the disease, only does harm by stimulating the part left to increased action. In the case of the breast, it is, I believe, impossible for any surgeon to be sure that he has removed every germ of disease unless he extirpates the entire breast; and this is the rule I always follow, though I am sorry to say it is not universally accepted. Dr. Hughes Bennett many years ago called attention to the fact that the microscope showed disease in tissues apparently healthy when in close proximity to diseased organs; and the late Mr. Charles Moore directed attention especially to the mischief of inadequate operations in cases of cancer, in a paper which you may consult in the fiftieth volume of the Medico-Chirurgical Transactions. To do your patient

justice, you must remove the entire breast in all cases, with all skin which may be the least implicated so as to have lost its healthy softness, any of the pectoral muscle which may be involved, and all enlarged axillary glands. The incision you should employ is, I believe a matter of perfect indifference, though in the majority of cases the common elliptical ones above and below the nipple are satisfactory. But you must vary your incisions according to the amount and position of skin involved; and, so long as you contrive that your wound shall have a good drain, its direction is unimportant. After the first incisions, I recommend you to make a very sparing use of the knife, and to trust very much to your fingers. You will find no difficulty, unless the patient is unusually thin, in stripping up the skin and its subjacent fat from the breast with your finger, aided by an occasional touch of the knife; and this plan has the advantage of securing you against slicing small portions of the breast or making "button-holes" in the skin. You can also better appreciate when you have reached the thin border of the breast, and so avoid leaving some of it behind, as is too often done. Forcibly raising the border of the breast, the surgeon at once ascertains whether the pectoral muscle is involved or not, and if it is, he should in my opinion, not scruple to cut into it freely, so as to remove, if necessary, a considerable portion of the muscle; and here again hemorrhage will be saved if the fibres are torn as much as possible. In removing the axillary glands, it is especially desirable to be chary of the knife, both on account of the hemorrhage and because the finger can hook them out so much more satisfactorily than any instrument. In cases of extensive glandular affections it is necessary to lay open and clear out the axilla almost as if for a dissection; and though, fortunately, the principal glands lie along the edge of the pectoralis, I have been obliged to expose a couple of inches of the axillary vein on more than one occasion, but without any harm resulting. It is common enough to expose the subscapular vessels and long subscapular nerve; but, by using the finger entirely, I have never had any trouble from hemorrhage. It has been proposed to cut across the pectoralis major in severe cases, so as to insure a removal of all the disease; but I have never found it necessary to do this, for it is, I find, perfectly possible to pass the finger up to and beneath the clavicle, and no more would be gained by the division of the muscle.

After completing the removal of a breast, I am in the habit of mopping out the wound very freely with the solution of chloride of zinc recommended by Mr. De Morgan, both on account of its antiseptic qualities and its possible effect upon any cancer-germs left in the tissues. I used to employ it of

the full strength (gr. xl. ad ʒ j.), but I fancied that it now and then caused the skin to slough, and I have therefore reduced it to one-half the strength, and I think, with advantage. In closing the wound, it is most important to leave a sufficient and dependant opening for drainage, and this is most conveniently placed near the axilla; and for a dressing I find nothing so satisfactory as oakum or carbolised tow, for the supply of real oakum has failed. This soaks up the discharge, and allows it to permeate, and hence keeps the wound dry, which I take to be the great point in favour of rapid healing and limited suppuration. It is convenient to place a piece of gauze between the skin and the oakum, so as to prevent the latter adhering, and to cover the oakum with a folded towel to absorb the moisture, which is abundant at first.—*Medical Times and Gazette, Feb.* 7, 1874, *p.* 147.

11.—ON THE RELATION OF URIC ACID TO GOUT.

By Dr. WILLIAM M. ORD, Assistant Physician to St. Thomas's Hospital.

Sydenham's brief but pithy definition of gout, as having its origin in the impaired concoction of matters both in the juices and parts of the body, is certainly out of harmony with the current ideas of the nature of this malady. Dr. Garrod's published opinions may be taken to be the best expression of these ideas; and it must be confessed no man has a better right to our respect than he, who, detecting uric acid in excess, as urate of soda in the blood, has since, by patient and conscientious observation, put this and similar discoveries beyond doubt. To examine Dr. Garrod's opinions:—In Reynolds's "System of Medicine," three propositions are offered by Dr. Garrod (p. 883): 1st. That to produce this form of articular inflammation the blood must contain urate of soda in excess, and even the phenomena of irregular gout, or gouty manifestations, require the same conditions. 2ndly. That gouty inflammation is invariably accompanied with the deposit of urate of soda in the inflamed tissue. 3rdly. That the amount of deposited urate holds no relation to intensity of inflammation; that in some structures infiltration may give rise to scarcely any inflammation, tending to show that the deposit is rather the cause than the effect of inflammation. In an earlier paper, Dr. Garrod spoke of excess of urate of soda in the blood and joint deposits as pathognomonic, so that gout might be defined from rheumatism as clearly as measles from scarlatina by the rash. The propositions, therefore, mark an advance in speculation; urate

of soda rises from a sign to be a *materies morbi*, and its deposit in cartilage or ligaments indicates these as agents in freeing the blood from excess; afterwards, the blood being still impure, other surfaces are selected, so that gout very nearly returns to its derivation—*i.e.*, a dropping (*gutta*) of morbid matter into joints. The cause of excess of urate is held to be partly defective excretion (kidneys), partly undue formation of uric acid in the blood. Dr. Garrod carefully guards against routine practice consequent on the simple acceptance of this idea, pointing out that each case exhibits its peculiarities, and demands, in some respects, separate treatment. This qualification being properly recognised, the conception of the cause of gout suggested in the above propositions is, as Dr. Gairdner always held, insufficient. The author maintains that it is most important to regard gout as a mode of decay, and a gouty man as journeying to the grave through definite regions of textural degeneration. Excess and deposit of uric acid are notable accidents of the way, but in errors of the entire nutritive process must be sought the explanation of that systemic loosening and poisoning to which gout may be reduced. Granted that uric acid with soda is deposited within gouty joints, does the fact necessarily mean that the acid is poured out to free the blood from its presence? The sum of evidence appears against the necessity of such conclusion. Uric acid, a product of disintegration of tissues, and food, is little soluble compared, say, with urea, and therefore the kidneys cannot remove much of it at a time from the blood. Similarly, if uric acid be largely formed within the tissues, the blood cannot take up much of it. In arcus senilis we do not infer the blood to be too full of fat, nor the cornea to be an eliminative organ; on the contrary, we infer degeneration of cornea, with comparatively insoluble deposit of fat within the corneal tissue. Again, cholesterine crystals in old sebaceous cysts, or in advanced atheroma, suggest local degeneration and immobility of resulting substance. Therefore, it is here claimed that urate of soda in parts of the body remote from the centre of circulation, in tissues little vascular and of low vatality, may be taken as a kind of degeneration or want of tissue organisation. Secondly, I would ask, is it necessary to assume that the deposited matter is either the cause or effect of gouty inflammation? As Dr. Garrod remarks, "Gouty deposit does occur in the cartilage of the external ear without any recognised preceding inflammation," and "in joints which have become callous, considerable deposits may occur without the production of much local inflammation." If, however, such deposits are meddled with ever so gently, sharp gouty inflammation always follows.

Taken deposits, or gouty constitution, or injury alone, no gouty inflammation follows. After death the great toe joint is constantly found the seat of unsuspected mischief—more or less ulceration (Dr. Garrod). The blood may be full of urate without producing gout, and the great toe may be injured again and again, and yet no gout appears. To show that the great toe is mainly the first joint attacked in primary gout, the author quotes Sir C. Scudamore's table of 516 cases, in which one or both great toes were affected in 341, or about two-thirds; in thirty-one, great toe with other parts of foot; and in only twenty of the whole was the lower extremity wholly exempt. In a word, the lower extremity, which bears the greatest strains in the exertions of daily life, is mainly the site of the first development of gouty inflammation. This holds good especially in hereditary gout; in acquired gout it is otherwise, as that means mostly general degeneration of body following default of particular organs. These organs are first attacked. So much for primary gout. Further, in our daily experience every fresh attack has mostly an exciting cause—a long walk, a joint-wrench, fall, nervous excitement or anxiety, &c., all disturb the circulation of some part; the temporary congestions of the healthy man become the characteristic inflammations in the gouty. In some cases the quantity of urate of soda seems to bear relation to the inflammation. Extension of gouty inflammation, however, requires not uric acid for its explanation. The author believes it may be propagated from part to part by nerve influence, as in simple inflammation. The direct influence of the nervous system in gout is admitted by all authors in one way or another In some cases a strong will can fight down the gout; others defy with impunity (for a time) the received laws of diet and regimen. Too much importance is attached to the supposed improvement of general health after an attack of gout. This is to a certain extent true in many early attacks, but recurring inflammations damage tissues and diminish power of recovery. To avert local attacks and check inflammation must be the aim of practice. The old rules of regimen hold good still. As regards remedies, eliminant medicines—and amongst them pure water—hold high value. In addition, all possible means to promote the concoction of healthy tissue—as cod-liver oil, milk, and fatty food, good meat, various tonics, as iron, arsenic, and vegetable bitters. We must try to wash away the offending extractive, not neglecting to repair and refit as perfectly as possible. *Summary.*—1. Gout is a mode of decay of the whole system, marked by deposit of urate of soda in and about joints, and by local inflammation of a particular kind. 2. The deposit of the urate is the result of local or general disintegration, and is not to be

regarded as a means of eliminating urate from the blood. 3. The local inflammations do not necessarily depend upon the deposition of urate, and the deposit is not a consequence of inflammation; at the same time it is probable that excess of urate in the blood produces irritation of tissues. 4. The local inflammation is peculiar, in respect of the ease with which it is produced, of the pain by which it is attended, and of the products—which are chemical rather than structural (chemical substances of low molecule, tending to crystallise or to be dissolved, being formed in the part, instead of substances of high molecule, tending to be organised). Interstitial sub-crystalline deposit is common, suppuration rare, in gout. 5. The local inflammations are set going by local exciting causes. 6. The local degenerations and inflammations tend to infect the rest of the system through the blood, and to set up similar actions elsewhere through reflex nervous action.—*Medical Times and Gazette, Feb.* 28, 1874, *p.* 233.

12.—ON THE EMPLOYMENT OF QUININE IN THE TREATMENT OF RHEUMATIC FEVER.

By Dr. A. W. BARCLAY, Physician to St. George's Hospital.

[Dr. Barclay states that his usual course of practice for some years has been to treat cases of rheumatic fever with large doses of alkali, his object being to neutralise the acid which is so abundantly formed in this disease. How far the effort is successful may be open to doubt.]

It can be readily shown in many cases that at the very time when the sweat-glands are stimulated to great activity, and a copious sour-smelling perspiration covers the whole surface of the skin, the kidneys may, under the influence of remedies, be secreting excessively alkaline urine.

It would lead me too far away from the main question were each of the points just alluded to discussed at the length which their importance demands; but there are one or two considerations to which I may call attention in passing. First of all stands out clearly before us this acid-forming tendency which so remarkably characterises rheumatic fever, and its special excretion by the skin and the kidneys. Uninfluenced by treatment, the urine indicates the presence of acid by its copious lateritious sediment, even sooner than the skin. It does not seem that as yet we can penetrate much deeper into the mystery than to say that in rheumatic fever the two conditions of acid formation and joint inflammation are associated, and invariably associated, together. Many other circumstances in the chemistry of life lead to the generation of acid. In

certain forms of dyspepsia, the patient may have his stomach full of sour fermenting aliment, and acid liquid rising into his mouth, with constant heartburn, and yet have no symptom of rheumatism. If we examine his urine while the stomach fermentation is going on, we shall find it neutral, or even intensely alkaline. A few hours later it has become excessively acid, with an abundant deposit of lithates. The acid has passed through the blood, and has been eliminated by the kidneys; but it has given no indication of its progress. Yet again, when the dyspeptic symptoms are very few and slight, the patient a free-liver, and ostensibly in the enjoyment of good health, there may be no period during the twenty-four hours in which the urine, if collected and allowed to stand, does not exhibit the same acid reaction, with the pink-coloured sediment adhering to the vessel. Such an one may have no rheumatic tendency whatever, though in this case too the acid which is being constantly excreted by the kidney is almost certainly formed in the stomach, and passed through the current of the circulation in order to reach its principal outlet. Mere acid formation is clearly not the cause of those symptoms to which the name of rheumatism is given, either in its acute or chronic form.

Next we have the striking fact of the kidney secretion getting rid alike of excess of acid and of alkali. It must be in obedience to some fundamental law of the economy that this result obtains. No more striking evidence of its reality can be adduced than the hourly variations of the acid and alkaline reaction of the urine seen in some of the forms of stomach fermentation just alluded to.

It is a little difficult to understand why for a time the generation of acid in the stomach should leave the blood in such a state as to produce excessive alkalescence of the urine; but it is clear that the acid is not wholly generated at the expense of the food. In addition to the fermentation excited there must be also a process of excessive acid secretion carried on by the glandular apparatus, and this leaves the alkaline base in the blood in such quantity that it cannot remain there, and it has to be got rid of by the eliminating action of the kidney. Hence the urine becomes for a time intensely alkaline; and if the acid pass out of the body by vomiting, it will necessarily remain in an alkaline state for a considerable period. If, on the contrary, the acid be retained in the stomach, and gradually passed on into the intestinal canal, the various absorbents begin to take it up in a liquid form; but as it can have no permanent place in an alkaline fluid like blood, the functions of the kidney are again called into requisition to get rid of the excess.

Further, these facts prove that although the blood is alkaline in health, there must be very considerable deviations on either

side from what may be called its normal degree of alkalescence under ordinary circumstances, and that still greater deviations are not incompatible with the continuance of life. Any substance having an acid or alkaline reaction, if absorbed from the stomach and primæ viæ, must in passing through the circulation to its outlet in the renal secretion cause such deviation temporarily to occur, its persistence being prevented by the tendency of the kidney to get rid of the excess whenever it is present.

The results of clinical experience can leave us in no doubt as to the reality of this eliminating power of the kidney; and it would seem to me that it must be regarded as one of its special functions. Physiology may perhaps some day explain why the structure of the kidney causes it to be the medium of separation from the blood of urea and uric acid to the exclusion of other glands in health. For the present we must be content with admitting that it is so. So long as the kidney remains unaffected by disease, these elements are not found in excess in the blood, their formation being exactly counterbalanced by their excretion. In disease, on the other hand, we find an excess of uric acid in the blood associated with gout, an excess of urea associated with Bright's disease. In gout there is no accumulation of urea in the blood; in Bright's disease there is no great excess of uric acid. But in either case, when the elimination by the kidney is stopped, the waste material may exude through some other channel.

The analogy between the function of the kidney as regards those two elements, which are necessarily regarded as its special secretion, and its power of removing from the body an excess of either acid or alkali, seems to me to be so striking, that I should be disposed to regard the one as not less important nor less clearly proved than the other. It is quite unnecessary to speculate on the remote effects of excessive alkalescence, or of possible acidity of the blood. Such conditions are of themselves abnormal—are, in fact, disease, The ordinary law of health is infringed; and as we find in all organised beings a tendency to return to the law of their life whenever it has been broken through, so we cannot doubt that in our own highly-developed organism there must be some means by which the law of normal alkalescence is preserved. This means seems to be provided in the kidney; and I think we are justified in concluding that it is not a mere passive outlet. It is quite conceivable that if the blood-serum became unusually charged with either acid or alkali, the excess would characterise every secretion, and that in the urine. the fluid passing by osmosis would differ in this respect from that ordinarily passed only in the same degree as that in which

the altered blood differed from healthy blood. But this is not so; and plainly such a result would fail to satisfy the requirement that the kidney should be a means of restoring the blood to a state of health. The intensely acid condition of the urine sometimes met with is an impossibility on the theory of its only passing by osmosis. The acid is actively and rapidly excreted, in order that the blood may be normally alkaline for the fulfilment of its ordinary functions.

Were further proof of this conclusion necessary, I think it would be found in the very remarkable fact already noted, that during the active and advancing period of rheumatic fever, if we pour alkali into the stomach to the extent of provoking free and rapid elimination by the kidney, acidity may still be traced in the perspiration. It has been usual with me to order the administration of the alkali every four hours at least; because if this period be exceeded, the urine will again become acid before the next dose is given. The kidney seems to seize the abnormal element in the blood, and expel it as soon as possible from the body. In order to reach this gland, the alkali must enter the current of the circulation, be fully mixed with the blood in the lungs, and distributed equally all over the body. Yet the sweat-glands may be secreting an acid fluid while the urine is intensely alkaline. My experience would rather go to show that when the perspiration has become alkaline, the disease is more under control, and is probably in the retrogressive stage; but individual experience is perhaps the most fallacious ground upon which an opinion can be based in the practice of medicine.

In speaking of excess of acid or alkali, I think it is not to be assumed that any such thing as free acid or free alkali exists in the blood or the urine. The reaction to test-paper is produced by salts in which the acid and the base do not fully neutralise each other, either because of their combining in varying proportions—one or more equivalents of base to one or more equivalents of acid—or in consequence of the reaction of one ingredient being too powerful to be neutralised by the other. The chemistry of life is so complex that we may not dogmatise or speak with too great confidence on a subject which cannot be brought to any scientific test. But it is a curious and somewhat inexplicable fact, that while there is this necessary elimination of alkali from the blood, in order to maintain its alkalescence at the normal standard, there is no restriction on the absorption of the alkali, which may be taken up in such quantity as to render the urine, and therefore the blood, alkaline to almost any extent. It seemed to me at one time that the solution of the difficulty might be found in another law of vital chemistry with which we are pretty familiar. By this law any

complex acid may be resolved, by its exposure to the oxygen in the lungs, into a more simple form; and when carbon enters into the formation of the acid, one at least of the simpler acids resulting from combustion will be carbonic acid, which forms, with the true alkalies, a very stable salt in the blood, having a very decided alkaline reaction. It is well known with what extreme rapidity a dose of citrate of potash will render the urine alkaline. The salt is readily taken up from the stomach, quickly burned into the carbonate, and rapidly eliminated by the kidney, to restore the blood to its normal degree of alkalescence. If, therefore, there were any check upon the absorption of a salt having an alkaline reaction, we could present to the absorbent vessels a neutral salt, which would have the same effect in increasing for the time being the alkalescence of the blood beyond its normal standard. It seemed not impossible in considering the subject that the absorption of an alkaline salt might be due to its entering into combination with acid in the stomach, and passing into the circulation as a neutral salt. The proof of such an hypothesis must be extremely difficult to obtain; and for the present it can only be stated as a fact, that in the form of an alkaline salt the alkali will pass in most cases as readily into the blood as in that of a neutral salt. In a succession of examples of acute rheumatism, I made the experiment with bicarbonate of potash alone, with citrate of potash alone, and with a combination of the two; and the result was, that on an average there was no difference in the length of time required to render the urine alkaline, so long as the cases selected were of equal intensity. When the disease was very acute, a longer time or larger doses were required to neutralise the acid. The conclusion forced itself on my mind, that however it may be explained, the alkaline salt is really taken up as such, and passes through the blood to the kidney unchanged, except it meet with any acid capable of entering into combination with it. From another point of view, the hypothesis that alkali could only be taken up in the form of a neutral salt seems also to be untenable. If it be assumed that free acid does exist in the stomach, and that its combination would aid in the absorption of the alkali, we should still be at a loss to explain how the urine was rendered alkaline, because it is only true of a limited number of acids that they become converted in the lungs into carbonic acid, and the salt found in the stomach would in many cases pass unchanged through the kidneys, and give no alkaline reaction to the secretion.

As a matter of inference, it can scarcely be doubted that while the citrate or bicarbonate of potash is passing through the blood from the stomach to the kidneys, if the urine be alkaline, the blood will also during the transit of the alkali be

in a state of excessive alkalescence. The question then immediately arises, whether such a result is beneficial or the contrary. Excess of acid is one of the never-failing accompaniments of rheumatism. How far it is subsidiary to other changes it is at present impossible to say: we only know that it is abnormal. From other sources we learn some of the evil effects of excess of acid, though not necessarily seen in all cases in which acid is present, and it seems rational to try to avert such consequences, if possible, by neutralising acid. With this view, we pour large and frequent doses of alkali into the blood, in the hope that it will be distributed to every part of the body; but we find that, in spite of all our efforts, with the exception of the kidney secretion through which the excess of alkali is being constantly expelled, the acid continues to show itself wherever we can search for it; the other secretions continuing acid in spite of the excess of alkali in the blood. The endeavour to neutralise the acid is clearly rational, but it may be questioned how far we succeed in attaining our object. In fact, as rheumatism is not merely acid but an acid-generating disease, so till the disease ceases the tendency will go on; and yet, after all, we are probably doing a real good, though we cannot by treatment succeed in arresting the acid formation. Notwithstanding the passage of an excess of alkali through the blood and a highly alkaline reaction of the urine, the disease will go on for some time unchecked—fresh joints become involved, and even cardiac inflammation may be developed. In fact, whatever may be said of the alkaline treatment of rheumatic fever, it cannot be called curative in the proper sense of the term. It may help to shorten the duration of the attack, it may serve to lessen the intensity of the inflammation, it may diminish pain, and it may not unfrequently ward off those more serious consequences which give to rheumatic fever its formidable character as a cause of heart-disease. All this it may do, and in my own opinion, and that of many others who have made the experiment, it does actually accomplish, but it does not cure. It does not put an end to the paroxysm, as bark puts an end to an attack of ague; it does not eradicate the disease, as iodide of potash will eradicate the traces of syphilis, commencing as soon as the system is sufficiently saturated by full doses of the remedy, and continuing till complete recovery takes place, with a progress unbroken as long as the remedy is used, retrograding as soon as it is left off. It does not even expedite the recovery in the marked way in which it is expedited when colchicum is administered in gout in a case not yet blunted to its action. Ere long I doubt not sufficient statistical evidence will prove its beneficial action; but as far as I can judge, it never can be capable of any logical

proof. I think that no one can fairly and impartially test it as a mode of treatment without coming to the conclusion that it is, on the whole, the best that has been suggested, but that it fails to cure the disease.

Among the large number of cases of acute rheumatism treated with alkalies, one cannot fail to remark that some of them give indications of depression, which can only be explained as the effect of the long-continued use of the remedy; and a question arises whether it is wiser to diminish the dose or risk the evil consequences of the depression, each having a tendency to make the case more protracted. Had we any positive rule by which to measure the quantity of alkali necessary, it would not be so difficult to decide; but there is certainly none such. Various means have been tried to meet cases of this kind. Stimulants avail to a certain extent, but have seemed to me to tend of themselves to keep up the disease, to lengthen its duration, and even to reëxcite it after it has subsided. Quinine, is that which, on the whole, has answered best in my experience, and it is chiefly to its value that I wish now to direct attention. It is somewhat difficult to define the meaning of the term depression as applied to cases of this kind. The pulse keeps up, there is no delirium, there is no collapse; but there is a general impression conveyed by the symptoms present that the patient is unduly depressed, and it has been observed that in such cases the duration becomes protracted, and convalescence does not set in so quickly.

It is then that I have given quinine with apparent benefit. One must not wait, as in slighter ailments, for the tongue to clean before its administration. It is given simply to meet this one symptom, because we know that there is worse behind. If the prostration be not removed, delirium will follow, and the case will at once wear a graver aspect. It is true that we do not now look upon delirium as evidence of inflammation within the cranium. The theory that the dura mater, being like the pericardium in physiological characters, must have similar pathological tendencies, has given way before the teaching of actual pathological facts that there is no such metastasis as used to be assumed. Still, when present, it indicates changes the gravity of which cannot be overlooked. The blood can no longer stimulate the brain to the performance of its normal functions, and life is in extreme peril. Hence the importance of tracing such a change at its earliest period, and arresting it before it has reached that stage of development when nothing can be done beyond pouring down large doses of stimulants.

In hospital practice it is very easy to watch the daily changes taking place in the urine in cases of this kind, and I have been struck with the apparent coincidence of nervous depression and

phosphatic deposit in the long continuance of the alkaline treatment. When first seen, the urine of a patient suffering from rheumatic fever is characterised by intense acidity, with a copious deposit of reddish lithates. After a few large doses of alkali, it becomes perfectly clear, but of a dark colour; as treatment proceeds, the tint resembling brown sherry changes to amber, and ultimately to straw colour, and sooner or later a deposit is seen which it requires no special chemical knowledge to designate as phosphatic, with alkaline urine. This coincidence has been so constant that I have come to regard the presence of such a sediment as the most certain evidence of depression.

Apart from the conclusion thus arrived at, that in such cases quinine was indicated with a view of correcting the depression of which the presence of phosphatic deposit became the sign, I had previously observed that in prescribing quinine, if there were any perceptible increase of the earthy salt prior to its administration, the deposit very frequently disappeared under its use. For some time the rule seemed so absolute that I did not meet with any exceptions More recently it has been seen that in a certain number of cases the quinine fails to arrest the chemical change.—*St. George's Hosp. Reports, vol.* 6, 1873, *p.* 101.

13.—ACUTE ARTICULAR RHEUMATISM TREATED BY ACIDS.

Cases under the care of Dr. WILKS, at Guy's Hospital.

In recently making some comments on the treatment of acute rheumatism, Dr. Wilks expressed a considerable distrust of the ordinary methods. He remarked that we do not yet possess an accurate knowledge of the essential cause of the disease, nor of the mode of its operation. We are, in fact, ignorant of the natural history of the disease. Medical practitioners are too apt to be credulous in matters of therapeusis, and are not sufficiently careful always to differentiate between effects and coincidences. Usually, for instance, the diagnosis of acute articular rheumatism is regarded as a sure and certain indication for the exhibition of alkaline remedies, and these remedies are persisted in in every condition or modification of the disease, without the slightest evidence of a beneficial influence resulting therefrom. Nay, it frequently happens that the more severe the disease the stronger does the belief in the particular remedy become, and medical men not uncommonly allege in support of the efficacy of the treatment that the complications were of the most serious nature. If the patient recover after having had severe heart disease or symptoms referable to an affection of the nervous system, it is regarded as proof positive of the

value of the remedy. Whereas we can not only not be certain that recovery did take place as a result of the treatment, but that it did not occur in spite of the vaunted remedies. On the other hand, some have asserted that there is no known cure for acute rheumatism, and that mint-water will answer every purpose. This is Dr. Wilks's opinion, and it was merely to strengthen this view that he gave mineral acids in the subjoined case, never supposing that they would do good or harm. Dr. Wilks maintains that, until we know everything concerning the disease, and especially the relation between the joint affection and the internal complications, we have no scientific basis for the selection of remedies. It is even doubtful whether the subsidence of the arthritic inflammation during the course of the disease is desirable, for in all fatal forms of rheumatism this is usually seen to occur. It may be remarked, however, that sometimes, in cases of internal complication, it does seem as if large doses of potash or of quinine have some influence in arresting the disease. In several cases treated by quinine Dr. Wilks has noticed the urine to become alkaline on recovery, as if this were the natural process of cure; as is seen also in typhoid fever. The same thing occurred in the present case.

For the following notes we are indebted to Mr. J. C. Ferrier.

G. W., a well-nourished man, aged thirty-five, was admitted on January 14th. A week before this he began to suffer pain in the hip-joints; subsequently both knees, and the right shoulder, wrist, and hand were affected. The pain in the joints was preceded by a feeling of coldness, but there was no actual rigor.

On admission the left knee was swollen from the presence of fluid within the joint. The right hand also was swollen and painful. A systolic bruit could be distinctly heard over the base of the heart, and extending a short way along the aorta. A venous hum was audible at the root of the neck. Perspiration was acid; temperature 101°; pulse 96; respiration 24. Tongue moist and covered with a white fur. The urine was acid, and contained a large quantity of lithates.

Ordered fifteen drops of dilute nitro-hydrochloric acid every four hours, and a diet to consist of milk, bread, and beef-tea.

Jan. 16th, Temperature 101·8°.

19th. Joints previously affected are better, but the right hand and both knees are still painful. Systolic basic bruit still very loud.

20th. Temperature 102·8°. Perspires freely.

23rd. Hands and knees almost well. Temperature 101·2°.

28th. None of the joints are swollen, but some feel stiff, and sore. Bruit hardly audible. Urine alkaline.

Feb. 2nd. Bruit almost entirely gone. The patient is convalescent.—*Lancet, February* 14, 1874, *p.* 231.

DISEASES OF THE NERVOUS SYSTEM.

14.—ON NERVOUS MIMICRY (NEUROMIMESIS) OF DISEASES OF THE SPINE.

By Sir JAMES PAGET, Bart., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

[Cases of nervous mimicry of diseases of the spine are nearly as frequent as the imitations of diseases of the joints.]

The chief things to study in the spine are pain, stiffness, weakness, and deformity.

As to pain,—spineache and backache, often not distinguishable, are perhaps the most common sufferings of the whole class of hysterical and other such nervous patients. Few escape them. But, then, similar pain is so common in other patients besides the nervous, and is found in so many and so various diseases, that pain alone can rarely decide a diagnosis. I must leave out of the question all the "backaches" that belong to lumbago, rheumatic gout, uterine, ovarian, renal, and other maladies, and try to answer for you only this: Are there any characters by which we may distinguish between pain that signifies real disease of the spine and pain that mimics such disease?

Well, we cannot always do so. There is no pain which is characteristic of real spinal disease—no manner of pain which may not be closely mimicked; but, in many cases, the nervous mimic-pain has characters which are not at all found together in real disease of the spine.

Nervous spineache is often described as very severe—raging, racking, as if the back were breaking, and so on. Now, such pain as this does not occur in real spinal disease, unless when the pain of some slight disease is immensely exaggerated in a nervous constitution, or in the acutest form of inflammation of the vertebræ—a very rare disease, always associated with serious general illness and impaired mobility of the spine. If there be no considerable attendant illness, an intense and horrible pain in or at the spine does not mean serious disease of the spine. I do not say that such pain is always merely "nervous" when it is the only wrong complained of. It may be due to aneurism, or cancer, or to some distant malady in a nervous constitution; but, unless in the two cases I just now mentioned, it is not a sign of spinal disease.

And this negative becomes the stronger when with such a pain there is excessive tenderness of one or more parts of the spine. Some tenderness on pressure of spinous processes may

be found with real disease of the spine or cerebro-spinal membranes; but excessive tenderness is not. This is rather characteristic of merely nervous disorder, of the so-called spinal irritation, and usually you find it, not at one, but at two or more parts of the spine, most frequently between the scapulæ and at the loin. At these tender spots the nervous patients cannot bear to be touched; they flinch and writhe when the finger taps or presses them very gently. You may be nearly sure that there is no disease of the spine when you see this, or when the tender parts of the spine are not painful in moving or in coughing or sneezing. And you may be quite sure, I believe, when a light blow or pressure produces more pain than a hard one, or when you find the same pain or flinching if the skin over or near the spine is pinched without pressing on the spine itself.

Again, the merely nervous pain is usually variable, though it may be never wholly absent; and its variations seem to be more dependent than those of real diseases are on disorders of distant organs, as the ovaries or uterus, the colon or rectum. In these variations the pain may seem paroxysmal, but I think it is not often so of itself.

Another usual character of this pain is its frequently extreme increase in fatigue, not only in bodily fatigue, as in long sitting upright or standing, but in long occupation of any kind, even in mental occupation if it is not very agreeable. With this character, also, it is common for the pain to be accompanied by nausea or even vomiting, and it is a nearly sure sign of merely nervous disorder if pressure on the spine produces shivering or nausea, or a feeling of going to be sick.

And, lastly, it is a sign of nervous pain alone if the pain has lasted many weeks or months, and nothing has come of it; no immobility of the spine or ribs, no paralysis, no fever, or wasting, or great disturbance of the general health.

I think you may be sure that a patient in whom you find all or most of these characters of pain in the spine has not spinal disease. But you will find many with pain like enough to this to excite suspicion, but not nearly like enough for conviction; many in whose cases the diagnosis must rest much more on other signs than that of pain. Among these, let me warn you that the greater danger is that of thinking cases to be mimicries of disease, or "muscular," or "only rheumatic," when they are really serious organic diseases of the spine or of parts within it. I will try to diminish the danger of being wrong in either direction by mentioning some of the more frequent groups of cases difficult of diagnosis.

The curvatures of the spine that occur frequently in young persons are often painless, are seldom very painful, and have

no characteristic pain; yet pain of any kind should always lead you to examine for curvature, and to suspect, if there be curvature already, that it is increasing. And the same rule should be observed with older patients; for it is not extremely rare for lateral curvature to begin after middle age, nor at all rare for it to increase at any period of life. In either case there may be pain, for which the spine should be often looked at for signs of disease more characteristic than the pain alone can be.

Aching spines are common in men with very nervous constitutions—in such as might be called hysterical with as much justice as many women are. Especially they are common in such men from thirty to fifty or more, and in women who retain their nervous or completely hysterical constitution to the middle or later times of life, and in whom, very probably, it is mingled with gout. There are far more cases of this sort in the cultivated classes of society than you are likely to suppose. Men and women of mature, or post-mature age, with spines whose nervous achings disable them from active life—at least from active bodily life; for in some of them the mental life is as active as if it really took their whole nervous force for its own use. They suffer severely, are painfully fatigued with exercise, have no comfort unless when they are lying down or reclining with their backs firmly supported; even slight manual work tires them: and yet in some of them the mind seems incapable of fatigue, or it only tells its fatigue in some increase of their spineache, or in feelings of nausea or vomiting. In many of these cases the diagnosis is not difficult: the nervous constitution is very strongly marked, and, after months or years of weariness and ache, you find the spine as straight, strong, and pliant as ever.

And yet among these are the chief cases in which you must be on the watch, lest you overlook a really diseased spine; for, among elderly persons, you may not only, as I have said just now, find cases of beginning or increasing lateral curvature, but, occasionally, one of disease leading to angular curvature, setting in very insidiously like a case of nervous pain or chronic rheumatism. The first sign of this is, usually, a constant aching, which is sometimes even severe, fixed to one part of the spine, or radiating from it, or extending round the sides; and there may be some tenderness. But, chiefly, you may know them by the diminished mobility of the affected part of the spine and the adjacent ribs. The patient holds the affected part of the spine as still as he can, when he turns or bends. If he coughs he does it carefully, and sneezing is very painful. If the dorsal portion of the spine be affected, the corresponding ribs scarcely move; if the cervical, pressure on the top of the head is often painful. After some few weeks of such pain,

yielding of the spine may become evident. If the patient is the first to notice it, he feels himself stooping, or obliged to support himself with his hands, or not so tall as he was; and, when you strip him, you may find two or three vertebral spines projecting in an incurable angular curve.

These things, you see, are much the same as those by which you recognise the progress of many cases of caries of the spine in children and young persons, and I suppose that this disease in elder persons is of the same kind. But I have not yet seen a fatal case or one examined after death; and I have only seen one in which abscess was associated with the angular curvature.

You may make another group of cases from other patients, in whom a strain of any part of the spine is followed by a very long abiding pain. Some of these will tell you that at the strain they felt, or even heard, something crack, and that they have had pain and weakness ever since—aching, wearing pain, increased by exercise, or in certain postures. Here again I must advise caution, especially when the injury is recent. In a large majority of these cases the long-abiding pain and other signs of spinal disease may be referred to the same nervous mimicry as we see in those whose sprained knees or ankles remain for weeks or months painful and weak, yet without disease. But in some the pain continues because, as may happen in an injured joint, the injury has been followed by inflammation; and this may be of serious form. The pain alone will not prove it; but you may be afraid of it when you find diminished mobility of the spine or ribs, great pain in coughing, greater in sneezing; and much more afraid when you find occasional or constant fever, and loss of weight and general strength. In short, for the diagnosis between the real and the mimic diseases following such injuries of the spine, you may study by the rules which are more easily learnt in the similar affections of the joints.

It is happy for our need of diagnosis that the impairments of the mobility and strength of the spine are more significant than its pains. The spine or any part of it, when really diseased, is often stiff and weak, though other signs of disease are absent or ill-marked; in the mimicry the mobility is often perfect, though other signs are vehement.

Fixity of the spine or any part of it is extremely rare in any nervous mimicry. I will not say that it is never seen, but it is so rare that when you find a patient carefully—not with muscular spasm, but with care—holding his head or neck or any part of his back very still, turning himself or bending cautiously, you must look for disease of the spine. There may be muscular rheumatism, or inflamed cellular tissue, or abscess, or whatever else may give pain in moving; but there is not likely to be only a nervous mimicry of spinal disease.

This fixity of spine is the more significant of real disease the less the attendant pain. Still more so is it when the ribs connected with the suspected and stiff part of the spine are also motionless or too little moved in breathing, and when the breathing is chiefly diaphragmatic. There may in this case be disease within the chest—perhaps the pleurisy that is often associated with acute inflammation of the dorsal part of the spine; or it may be very uncertain what disease of the spine there is; but it is very nearly certain—as certain, I think, as any diagnosis of unseen disease—that there is not a mere mimicry. Whenever you are told of "spinal irritation," "hysteric spine," or whatever else an unreal disorder at the spine may be named, look carefully to the mobility of the spine and ribs; if it is impaired, look much further before you venture to conclude that the malady is only nervous.

The very opposite state of the spine, in which it is all limp, so that when the patient tries to sit up, he, or more often she, bends or tumbles this way or that, like a baby, is nearly certain to be without organic disease. There may be real paraplegia; if there be not, you may believe the weak tumbling spine is in itself healthy, though it may contain an idiotic spinal marrow; as a good skull may hold very foolish brains.

A feeling of weakness or giving way at one part of the spine is ambiguous. It is often complained of in neuromimesis; but it is also often present in carious or other real disease of the spine. It must always be considered likely to be a grave symptom if the patient habitually, and almost unconsciously, helps to support the spine with the hands or elbows as he sits. Of course this self-supporting posture may be either unintentionally or on purpose imitated in a nervous mimicry; but it so often has a real meaning that it must not be made light of, unless all other evidences of mimicry are clear.

You will often find that with this weakness of one part of the spine there is some distress in moving it. The patient does not willingly rise or turn in bed, and if he walks he does it slowly, often stooping or leaning forwards, going stiffly or shuffling, not staggering or unsteady, unless the spinal marrow be involved in the disease. This sign is, I think, less rarely mimicked than the last; the two together are weighty evidence for real spinal disease, and if defective movement of the ribs be added you had better believe that the spine is certainly diseased; whether with struma or rheumatism, or whatever else.

The other leading sign for the diagnosis between real and mimic diseases of the spine is in its shape. Is it misshapen, wrongly curved, or in any way deformed? If so, it probably is or was really diseased; and yet even here there is room for error.

An angular curvature of the spine—I mean such backward outstanding of one or more vertebræ as is due to thinning or loss of substance of their bodies or intervertebral discs—is, I believe, quite inimitable by any nervous or muscular condition. But in not a few persons you will find that one or two vertebral spines naturally project a little, or are placed a little to one side of the exact line or curve in which they should be. This is most often seen in the lower dorsal and lumbar part of the spine; but it is so little like disease that it would be unnoticed if spineache or some fright about curved spine did not call attention to it. I think you will seldom have any difficulty in distinguishing this natural error from any effect of disease.

A lateral curvature of the spine may be imitated by disorderly muscular action; not, indeed, perfectly, yet nearly enough to be often deceptive. Spasmodic wry-neck is well enough known, and may imitate the distortion of considerable disease of the cervical part of the spine. Lateral curvature of the dorsal and lumbar parts from similar muscular disturbance is much rarer, but you may expect to meet with it; and you may often detect the mimicry by noticing that the curvature has formed very quickly or even suddenly, and has become marked or extreme in so short a time as could not have sufficed for a real lateral curvature. A few days will make an imitation-curvature stronger than as many months will make a real one. Besides the mimic curvature is not a perfect likeness. It is sometimes single, though very marked; real lateral curvatures, if very marked, are at least double; and the vertebræ are little or not at all rotated, as they are in well marked real lateral curvatures. If these signs of distinction are not enough, ether or chloroform will help. You can straighten the mimic curvature when the muscles cannot act; you cannot so straighten a real curvature.

Other deformities of the spine may be imitated by hysterical and other such persons. The spine may be rotated without curvature, or drawn to one side, so as to go up obliquely from the pelvis; or in those with very weak, limp spines there may be, when their trunks should be erect, an appearance of an uniform posterior curvature. But I believe you may easily detect the mimicry in all these. Most of them are extravagant, going beyond almost any real deformity of the kind that such a patient could have; and, almost always the wrong may be righted by putting the patient in some unusual position, as lying very flat on the back or front, or with the hands touching the feet, or hanging on a swing. By these, or the like means, you may out-trick the trickery of the muscles.—*Lancet, Nov.* 29, 1873, *p.* 763.

15.—ON THE NERVOUS MIMICRY OF TUMOURS.

By Sir JAMES PAGET, Bart., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

[There are three principal forms of nervous mimicries of tumours, viz., the muscular or phantom tumours, the imitations of aneurisms, and those of cancer.]

In what are called phantom tumours the imitation is due to the swelling of part of a muscle during contraction. This part, swelling and becoming harder than the rest, feels something like a smooth round or oval tumour or cyst, placed in or between the muscle. The rest of the muscle feels natural, whether relaxed or contracted. In the former state of the muscle the apparent tumour is most distinctly felt; in the latter state it may be nearly concealed, as if sinking in the muscle.

This condition of muscle is altogether a strange one—not imitated, so far as I know, in any other state of muscle, unless it be in crimping, or in the flickering and passing contractions of dying muscles, or in the lumps which, it is said, may form in muscles that are struck soon after apparent death in any acute fever. If it may be compared with anything in living muscles, it may be with cramp which draws them into knots or lumps; but there is no pain in phantom tumours as there is in cramp. Whatever it may be, the condition seems due to erroneous nerve force in the muscle; for one form, the phantom tumours of the abdominal muscles, is most frequent in evidently hysterical women, and in other cases that I have seen there was certainly an abnormal state of nervous system, and in others it was very probable.

There should be no great difficulty of diagnosis in the ordinary cases of this kind. A real tumour that lies deep in or beneath thick muscle may be pressed down beyond clear touch when the muscle contracts and hardens; and it may be difficult to feel the boundaries of a venous tumour or nævus in a deep or thick muscle. But these things will not mislead you if you will study the case long enough to be clear as to the inconstancy of appearance. If in one minute an apparent tumour is under the fingers and in the next minute is gone; if it shifts from one part of a muscle to another, as one set of fibres after another contracts; if it wholly disappears when the muscle is long relaxed,—this is a phantom, a mere mimicry of a tumour.

There is rather more difficulty in some of the cases of phantom tumours which occur in the abdominal muscles of hysterical women. These are sometimes large, hard, and more

nearly fixed in place, size, and form, than those in the limbs, and they may be deceptively complicated with disorderly states of the intestines, or the aorta, or other abdominal or pelvic organs. But in any case, however difficult of diagnosis, ether or chloroform will bring all the help you need. With complete insensibility and muscular relaxation all signs of tumour disappear—the phantom vanishes.

Let me tell you the most striking case of this kind that I have seen. It was in a healthy-looking woman, about thirty years of age, in Sitwell ward. I admitted her for a tumour in the upper part of the abdomen, behind, as it seemed, the first division of the left rectus abdominis muscle, but larger than that seemed. It was roundish, firm, nearly hard, constant in its characters and place, often painful, and distinctly pulsating, like an aneurism. A light bruit was audible in it. Of its history I only remember well that there was nothing to help in diagnosis. In full medical and surgical consultation with my colleagues, the questions were whether there was a tumour with pulsation communicated from the aorta, or an aneurism. If a tumour, where, or in what? If an aneurism, of what artery? The opinions were many and various, partly, perhaps, because the examination was painful, and, therefore, possibly, incomplete. So one day I gave the patient chloroform, and the tumour, the aneurism, and the doubt dispersed; they were all phantoms.

This case, in which an abnormal nervous condition of a muscle and of the abdominal aorta appeared to be combined, may lead us to the next set of mimic tumours—the imitations of aneurisms by what I suppose to be abnormal nervous conditions of arteries. The evidences of this condition are, indeed, not complete; but, at least, the mimicry of aneurism is more frequent in persons of nervous constitutions than in any other. It is sometimes associated with well-marked hysteria, and the likeness of aneurism often varies, according to the state of the patient's nervous system.

This mimicry is most frequent in the abdominal aorta, in which it has often been described as a nervous abdominal pulsation; but, so far as I know, it is not described as occurring in the subclavian, innominate, or carotid arteries, though in those it is not very rare, and sometimes is not easy of diagnosis. The chief characteristic of the mimicry is that the affected artery feels much larger than it should be, and pulsates largely at least in one direction, dilating obtrusively, and often so much more visibly than in an ordinary person that it is hard not to believe that it is largely dilated. And yet there certainly is no considerable dilatation; it is rather as if the arterial walls were thin and had too little muscular resistance,

as we might suppose them to be in a condition of partial paralysis of their muscular tissue. In the most marked case of imitation of abdominal aneurism that I have seen, examination after death found no disease. A lady of very nervous constitution had suffered severe sea-sickness in a passage to Ireland. The sickness scarcely ceased during her stay there, and was aggravated on her return voyage, after which she continued vomiting nearly all her food, till, at the end of some weeks, she died exhausted. During these weeks there was so large and strong a pulsation in her abdominal aorta that some were convinced that she had abdominal aneurism. As she lay on her back, one could see the artery pulsating behind the abdominal wall. It raised the hand laid on it, thrusting up the fingers with a firm throb and a thrill, and a low but distinct blowing was audible when the stethoscope was lightly pressed on the artery. Yet there was no aneurism. After death the artery was found absolutely healthy in both size and structure.

I say again, I do not know what is exactly the state of the arteries in these cases. There are few opportunities of examining them after death; and I have heard of none that told more than the one I just now mentioned to you. But, clinically, these cases are well marked. The artery feels large, wide, and full; but soft and compressible, or even, if one may so call it, puffy, without any of the hardness or stiffness constantly found in aneurisms. The pulsation is full, but rather soft—like that of an artery in the reaction after large loss of blood; and, which is chiefly characteristic of the absence of dilatation or aneurism, the extra full pulsation is in only one direction. There is no unusual lateral dilatation; the too much throb is only forwards.

The likeness to aneurism in these cases is sometimes greater than could well be expected; for many things besides the excess of pulsation may contribute to it. In the abdominal aorta an unusual appearance of prominence may be given by an unusual anterior curve of the lumbar vertebræ; and by thinness with concavity of the abdominal walls, and emptiness of the stomach. Hardness or toughness of the pancreas, or of lymph-glands about the aorta, may yet further increase the likeness to abdominal aneurism.

In like manner, the imitation of subclavian aneurism is made more marked when a nervously pulsating subclavian artery has glands beneath or about it; and still more when it lies on a cervical rib, and is somewhat raised and, I suppose, widened. And the imitation of upper carotid aneurism is added to by the very bulbous form of the first portion of the internal carotid and its occasionally tortuous course. That of

the lower carotid aneurism may be augmented by an enlargement of an overlying lobe of the thyroid gland.

With all those helps to difficulty you may sometimes be troubled in the diagnosis of a mere nervously pulsating artery. In nearly all the cases I have had to see aneurism was suspected; but a few rules will ensure against error. The nervous artery has no lateral expansion; it does not open the finger and thumb when they laterally compress it lightly; you can trace the straight lines of its sides; in the abdominal aorta the pulsation is lost, or nearly lost, when the patient sits leaning forward; in the other arteries by relaxing the parts over them. Always the artery has at least its natural softness and compressibility; commonly it has more. There are no paroxysms of pain: and if the case has been watched for months, or even years, there is little, or more often no, increase of size. On the other hand, the extent of artery which may feel dilated may much increase, so that we may see and feel, for instance, an abnormal throbbing in the innominate, carotids, and subclavians of the same patient—a state unheard of in aneurisms.

Before leaving this subject, I just mention to you the cases in which you will find, in certain patients, anæmia, enlarged thyroid gland, protruding eyeballs, and pulsating carotid arteries. The study of these cases might clear the obscure pathology of the mimic aneurisms; but with this I am not occupied, so I leave them, and go on to the last set I have to speak of.

It may seem absurd to say that cancerous tumours can ever be imitated by any nervous disorder; and, indeed, they cannot; yet you will find few cases requiring a more positive diagnosis than those in which you will have to assert, as beyond all doubt, that a patient has not cancer, but neuralgia.

The cases in which the diagnosis is most often necessary are those of suspected cancers of the breast and of the tongue. Of course any part may be similarly suspected; but, taking all other parts together, they would not supply, at least in surgical practice, so large a number of cases of suspicion as these two.

For the breast, the usual case is that a patient complains of all the pains commonly described as characteristic of cancer. She has dull, aching pain, it may be, of the whole or some part of the breast, and often the pain extends down the arm, more often goés right through to the scapula; and sometimes it is a darting, lancinating pain, shooting this way or that, or burning. I think that you will not find any pain of cancer of the breast which will not be described by patients with this neuralgic mimicry of the disease.

But, however severe the pain, and however like what is

supposed to be characteristic, your diagnosis may be clear. That is not cancer which you cannot feel. Cancer is a growth, not any kind of pain, and indeed in its early stages seldom attended with pain; so that when pain is strongly marked, and it is very difficult, or impossible, to feel any tumour or "lump" in the breast, you may be certain that there is no cancer.

In most of these cases the pain has an entirely mental origin. The patients have been seeing or hearing of cancer, and their minds have been filled with thoughts of the pain and misery of the disease, till the idea has generated the sensations from which it was derived, and the subjective sensations are referred from the centre to the periphery.

I have said "the mind has been filled;" but this is not necessary. I have known these pains mimicking the pains of cancer in persons who had indeed often thought of cancers, but never intently, nor with any sustained fear. Just as one may, in dreaming, have vivid impressions of objects he has rarely, perhaps only once, seen, and scarcely ever thought of, so may these imitative pains be felt keenly and often in those who have seldom considered or feared them.

In these wholly mental cases the confident assertion of your judgment will commonly suffice for cure. Sometimes, indeed, patients need repetition of the assertion, especially those who are hysterical, &c. Their trust seems to die out after a few weeks or months, and some of them rather prefer the disease.

There are cases, however, in which the pain is not wholly mental. Neuralgia of the breast sometimes follows a blow. That which in ordinary persons would be followed by aching for some inconsiderable time is apt to be followed in the nervous by long-abiding pain, which the mind will invest with all the characters of what it supposes to be the pain of cancer. And, again, neuralgia of the breast is not rare in gouty or otherwise "painful" persons, and most of these are ready to fear that their pain is due to cancer, and to endow their pain with the characters which they suppose to belong to it. As fear can give to any harmless form the shape and substance of that which it most dreads, and can see spectral terrors in a scarecrow, so can it give to ordinary neuralgic pains any of the characters of the pains of cancer. It is especially prone to do this when the neuralgia is felt in a part notoriously liable to cancer. In this part fear will make pain cancerous, even while neuralgic pains, quite as severe, are common in other parts of the body, and in them are regarded without dread.

The diagnosis is more difficult when, with the pain described as like that of cancer, there is, or appears to be some change

of structure of the breast; for of course a very nervous patient may endow any mammary disease with any form of pain.

In these cases—which, however, are far more rare than those of neuralgia without change of structure—you must make your diagnosis on the characters of the tumour itself, if there be one. But be cautious about one or two fallacies. Many neuralgic breasts are a little larger than their painless fellows. But mere largeness is not tumour, much less cancer. And many mammary glands, whether neuralgic or not, are rather firmer or tougher in one part than in others; and when the tough part is grasped, and, as it were, doubled between the thumb and fingers, it may feel something like a hard tumour. Let me advise you not to use this mode of examining a mammary gland. I have known it often fallacious, never useful. If you cannot feel a tumour by feeling or pressing over and round it, you may believe there is not one; you may be certain there is not a hard cancer.

And if you can feel a tumour, do not believe it to be cancerous merely because it is painful. Judge by what you can feel, not by what the patient feels; remembering always that in the early stages of cancer of the breast, in which alone there is any difficulty of discerning it by its tangible characters, it is rarely very painful. All the popular ideas of the pains of cancer are derived from the cases of advanced disease, or of cancers growing rapidly or inflamed or ulcerating. In early cases, and in nearly all in which the diagnosis is obscure, the greater the pain the less is the probability of cancer. And, let me add, neuralgia, even of the severest kind and longest duration, does not tend to cancer of the breast. I must have seen more than a hundred cases of neuralgia, and I have seen only one in which it was followed by cancer, and I have no belief that in this one the sequence was a consequence.

Most of what I have said of the breast might be applied, with change of name, to the mimicry of cancer of the tongue. Nervous people hear of the cancer, and then, with less or more of thought about it, have pain and aching in their own tongues, usually at the side and in that frequent seat of cancer, at the junction of the middle and posterior thirds. Others have neuralgia of the tongue; a dull, heavy, aching on one side of it—a condition which is far from rare in the habitually neuralgic, especially, I think, in the gouty, and these give to their neuralgic pains what they believe to be cancerous characters. And others, with dyspepsia, have sore tips or edges of their tongues, which they, too, magnify.

It is strange to observe the tenacity with which some of these patients cling to the most dismal view of their cases. Though nothing wrong can be seen or felt, and though months, or

even (as I have known) years, may pass without any disease appearing, yet will they believe themselves on the brink of misery with cancer of the tongue. But your diagnosis may be as fixed as their fears, and had better be more positive. That is not cancer which you can neither see nor feel. And do not be deceived by an unusually large cluster of papillæ of the circumvallate group, or by any little thickening due to irritation of a tooth, or by a fissure or psoriasis. A patient's fear will give any of these the sensations of cancer; but, as I said of the breast, so here, your diagnosis must be founded, not on the patient's sensations, but on your own.—*Lancet*, *Dec.* 13, 1873, *p.* 833.

16.—ON FEIGNED HEMIPLEGIA.

By Dr. ALEXANDER DAVIDSON, Physician to the Liverpool Northern Hospital, and Dr. CHAUNCY PUZEY.

The subject of feigned hemiplegia has been only very cursorily alluded to either in standard works of general medicine or in writings specially devoted to the consideration of feigned diseases. So far as we are aware, no detailed history of any such case has been recorded in medical literature, and we think that the following well-marked example will be both interesting and valuable.

Not long ago we were requested to visit, on behalf of a railway company, and to report upon the case of a young man who was believed to be suffering from hemiplegia, the result of a railway accident. The history we received was as follows:—

Eight months previously in a collision, he had received a blow on the left side of the body, which was stated to have been followed by hæmoptysis and the appearance of blood in the urine and motions. For this he was leeched, poulticed, &c., and remained under medical treatment for five weeks, when he was brought to Liverpool. At this time no symptoms existed of any injury to the brain or spinal cord; but two months after the accident, we were informed, he began to complain of pain in the dorsal region of the spine and of numbness of the left leg. He was kept in bed under medical observation; and one night, four months after the accident, he was found doubled up in bed, staring about, struggling and shouting. This attack was stated to have continued for seven hours, and he required several men to hold him still. Similar attacks recurred every night for a week following. After this the left leg became powerless, and weakness of the left arm was noticed; and the symptoms gradually increased until he appeared to have total loss of sensation and of motion on the left side.

We saw the patient in the presence of his medical attendants, and made the following report on his condition:—

We found the patient in bed, lying on his back. His countenance was rather pale, but did not exhibit signs of serious ill-health. The pulse was somewhat rapid, but regular and of fair strength. The body appeared well nourished. The heart, lungs, &c., were examined and found normal. There was no facial paralysis; the tongue was protruded normally, and articulation was perfect. Memory and intelligence appeared unimpaired, his answers to all questions being ready and sensible. The pupils were normal, and the vision appeared good. His own statement was that he suffered from headache, that his memory was not so good as formerly, and that his vision and hearing on the left side were somewhat impaired. In all these respects, however, he said that improvement had now begun. He admitted that he usually slept well, and said that he occupied a considerable portion of his time in reading. He exhibited signs of much tenderness when the lower dorsal vertebræ were touched, and complained of pain extending from that locality towards the left side; but he said he had no feeling in the skin all over that side of the trunk up to the clavicle, and pricking with a needle failed to elicit signs of pain. When raised in bed, he was able to sit erect with both his hands in front of him, and without assistance. The skin of the back was sound, and there was no bedsores. No paralysis of the bladder or rectum existed. We were told that the bowels acted three times a week, and that on those occasions he went to the closet outside his bedroom, with the assistance of one person.

On proceeding to examine his left leg, which was stated to be completely paralysed both with regard to motion and sensation, we found no difference in appearance between it and the right limb. The muscles on each side were equally firm and well developed, and the measurement of the two legs corresponded in every particular. He professed to be unable to move the left leg to the smallest extent, but when the leg was raised from the bed and bent backwards and forwards, distinct muscular contractions were felt by us. On applying a weak faradaic current, we found the electro-contractility of the muscles perfectly normal. On pricking different parts of the leg with a needle, no sensation appeared to be excited, and no reflex movements were caused by this or by tickling the sole of the foot. On one occasion, however, at a later period of the examination, when his leg was surreptitiously pricked under the bedclothes, his attention being drawn away by a feigned examination of his eyes, a decided jerk of that leg took place; at another time, when pricked unexpectedly on the inner side of

the left buttock (as he lay upon his face), he flinched most decidedly; but after this was openly remarked on and the pricking was repeated in the same spot, no movement occurred.

In order to observe his mode of progression, he was had out of bed by the aid of his brother. He then stood on his right leg, with the left leg slightly bent at the knee, the heel drawn up, and the toes scarcely touching the ground. He then proceeded towards the door, supported by his brother and holding on to the bed. With this help he made a series of steps with the right foot, and at each step we observed that the left leg was jerked up (by muscular action) just as occurs in hopping on one leg. The dragging of real paralysis was wanting. As he was getting into bed again the knee of the affected side was bent almost at a right angle, before it touched the bed.

The paralysis of the left arm was stated to be complete with regard to feeling, but incomplete as to movement. Both arms appeared equal as to size, muscular development, and temperature. On pricking the left arm with a needle, no pain was complained of, and no reflex movements were excited. On being asked to show to what extent he could move that arm, he raised the forearm slightly, but used the help of his right arm to raise it at the shoulder. The muscles contracted well under the faradaic current.

He was again seen by us after a lapse of three months. The general characters of his condition remained, with the following alterations. The left arm was stated to have recovered much of its sensibility, and the power of movement had partially returned. He was also able to move the toes of his left foot to a slight extent, and said he had some feeling in them. He still appeared to be unable to stand or walk. A marked difference, however, was noticed in his manner of progression from what we had previously observed. The heel of the left side was still slightly drawn up as he moved, but now the toes were dragged along the ground, the right knee being also slightly bent. The following circumstances were noted as he lay in bed. On attempting to bend the knee while we elevated the leg, he at first forcibly resisted, the extensors of the thigh and leg being felt to be strongly in action; after a few seconds, however, they were suddenly relaxed. The skin of the thigh seemed to be as insensible as before, the patient even running a needle into different parts of it himself, to show how little he felt. When turned over on to his face he appeared to forget which was the affected side, for, seizing his *right* buttock and pinching it hard, he declared he felt nothing. On his mistake being referred to, he insisted that it was the other side which he had pinched. The impression left by his fingers, however, remained evident on the right buttock. The feet were clean, but the soles were

dirty, as if used on the ground (though he stated that he had been absolutely confined to bed for some months). The skin of the soles was smooth and hard, and there was no appearance of that crackling or scaling of the cuticle which we see in the soles of adults who have been confined to bed for several months.

The conclusions which we arrived at as the result of these examinations were: That though some injury of the side had been received in the accident, its effects had entirely passed away; that no disease of the brain or spinal cord existed; and that the hemiplegic symptoms were feigned.

The case was tried in one of the law courts. The medical attendants of the patient and a physician (who saw him shortly before the trial) gave evidence that in their opinion the paralysis of the arm and leg was real, that it was due to injury of some of the nervous centres, and that the man was probably permanently disabled. In consequence of the difference between their medical opinion and ours, a further examination of the plaintiff was made during the trial, in the presence of another physician and a distinguished surgeon; and on this occasion some entirely new features appeared. For the first time, loss of sensation was complained of in the left side of the face, neck, and tongue, but without any dragging of the face or deviation of the tongue when protruded. The mode of progression was again different, the dragging of the leg being much more exaggerated than before, and the patient making no effort to support himself on the sound leg, but allowing himself to fall down if unsupported. It was also noticed that when the patient's eyes were closed the apparent line of insensibility, as tested by the needle, varied from a quarter of an inch to an inch to the *right* of the median line. The opinion given at the trial by the two gentlemen who had now seen the case for the first time was that it was a case of hysterical paralysis; but nothing had occurred in this third examination to shake our opinion. However, the jury gave a verdict for the plaintiff, with substantial damages.

Not very long after the trial he was seen by one of us walking about apparently quite well, and when spoken to, admitted that he was "nearly well, thank God."

To the medical practitioner in ordinary or in hospital practice such cases of feigned disease do not frequently occur; and when they do appear, he has usually ample opportunities for detecting and exposing the fallacy of the symptoms. And for this purpose there are three methods open to him. Firstly: The observation of the character of the symptoms, and the comparison of them with cases of undoubted disease. Secondly: Having the patient watched without his knowledge, so as to detect any movements or actions inconsistent with his supposed ailment.

(This is of course best managed in a hospital.) Thirdly: The employment of violent or disagreeable methods of treatment, calculated to weary or disgust the patient; and even these may be resisted for a long time.

But it is different if the physician has not medical charge of the patient, but is called in under certain medico-legal circumstances, to form his opinion on observations made by him in one or two examinations. Then the first only of these methods is available to any extent; and it is only by a minute observation of the symptoms and general features of the case, and a careful comparison of these and of the statements of the patient with the known characters of real disease, that he can hope to arrive at a correct conclusion.

And in the first place, the general combination of symptoms, and their consistency with one another, should be considered.

In our case, the total anæsthesia of the left limbs and left half of the trunk, combined with the nearly complete paralysis of motion in the limbs of the same side (the face being unaffected), formed a group of symptoms which it would be extremely difficult, if not impossible, to explain on pathologico-anatomical grounds.

Secondly, the real nature of the defective sensibility must be tested.

It is well known that anæsthesia has often been feigned for various purposes. Numerous such cases have been recorded, especially by military surgeons, where much greater pain than the prick of a needle has been borne with the most unflinching resolution, and without showing the least sign of feeling. In the instance just recorded it was evident, from the involuntary movements which were on one or two occasions caused by the unexpected application of a needle (when the man had not his muscles under control), that he really had tolerably acute sensibility. Where treatment by powerful shocks of electricity is available, it will usually be easy to cause such pain as will lead to the exposure of feigned anæsthesia.

Thirdly, the symptoms of motor paralysis must be carefully investigated; and if the paralysis is complete, or nearly so, it will not be difficult to distinguish between the real and the feigned disease.

Several tests are available. 1. The well-known appearance of dragging in a paralysed leg. This symptom was at first entirely absent in the above case; the slight dragging which was subsequently observed differed from that of ordinary paralysis. (See report.) 2. The test suggested by Dr. Hughlings Jackson. In real hemiplegia the paralysed arm falls forward when the patient stoops; in case of feigning, the arm will probably be

retained by the side. (We omitted to use this test in our case.) 3. The condition of the muscles in long-continued paralysis. Where paralysis has continued for several months in a limb, we should expect that the muscles would both diminish in size and lose their electro-contractility. Neither of these conditions existed in our case. 4. In cases where the patient says that he has been confined to bed for many weeks, the condition of the soles of his feet may be taken as a test of the correctness of this statement. A scaly and cracked condition of the cuticle of the soles will, according to our experience, be invariably found in adults (at least in the lower ranks of life) who have been confined to bed for a considerable time.

In all cases of suspected feigning of disease we should endeavour to ascertain whether the patient has had opportunities of becoming acquainted with the symptoms of the disease supposed to be simulated. In our case we were informed that an acquaintance of this man, who was in the same accident, became actually hemiplegic within twenty-four hours after meeting with his injury, and that this fact was well known to the patient.—*Lancet, April* 4, 1874, *p.* 474.

17.—THE DETECTION OF MALINGERING BY ELECTRICITY.

By the EDITOR OF THE BRITISH MEDICAL JOURNAL.

Malingering for the purpose of extortion, or for procuring the means of subsistence without work, or merely for exciting compassion and interest, is much more frequent than is generally supposed. Where the physician's suspicions are aroused, faradisation of the skin with a wire brush and a powerful current, may be an excellent means of settling such questions. This proceeding is extremely painful, and yet never does any harm, except in cases of cerebral disease, where it should be avoided. We need, therefore, not be reluctant to employ it in suspicious cases, as it is far more humane than the actual cautery which has often been called into requisition, and yet sufficiently disagreeable to make a repetition of the procedure dreaded by such persons. The Welsh fasting-girl might still be alive, and in the enjoyment of a hearty appetite, if her case had been looked upon in a proper light, and if faradisation of the skin had been a few times employed as a diagnostic and curative measure. Faradisation may also be usefully employed where certain forms of paralysis are assumed by the malingerers. We have by this means succeeded in unmasking a person who intended to extort money from a working-men's benefit society, on pretence of having suffered paralysis from an

accident. As in peripheral paralysis the muscles always lose their faradic excitability, the presence of the faradic response in the muscles of the arm enabled us in that case to state decisively that the man was shamming.—*British Medical Journal, Nov.* 22, 1873, *p.* 614.

18.—A CASE OF SUPRAORBITAL NEURALGIA RELIEVED BY SUBCUTANEOUS INJECTION OF STRYCHNIA.

By R. J. A., a Correspondent of the Lancet.

Mr. A., who had been for some time a resident in a climate in which ague was prevalent, and who had an attack of that complaint, laboured under a severe attack of supraorbital neuralgia. It was distinctly intermittent, and considerably affected the general health. On seeing him, he said he had eaten very little for eight days. Two minims of solution of strychnia (four grains to the ounce) was injected into the right arm. Two days afterwards he told me he had slept well; his appetite was better; but there had been some slight return of the pain, and at the time of the second consultation he felt the supraorbital pain, though somewhat diminished. Two minims of the solution were injected, and the pain instantly disappeared. After a third hypodermic injection the pain disappeared. Since then he has been in good health, the injection in the last instance being followed up by administration of iron and quinine as double citrate. It is right to mention that a quinine treatment was adopted without a favourable result.

Were it not that we had the history and the unilateral character of the disease, it might be supposed that the effect which strychnia as a subcutaneous injection has on the stomach would act thus in removing the pain. The most interesting feature of the case, I conceive, is the immediate disappearance of the pain on the hypodermic injection of the strychnia.—*Lancet, April* 11, 1874, *p.* 533.

19.—PHOSPHORUS IN NEURALGIA.

By S. Messenger Bradley, Esq., Manchester.

My custom is to commence with one-hundredth of a grain, and gradually increase this by one-fiftieth of a grain at a time, until, if necessary, one-tenth of a grain is taken with each dose. Beyond this quantity I do not go; as I think that, if the remedy be of use, relief will be attained by this dose equally with a larger. After trying several preparations, I now use a formula which Mr. Potts, dispenser to the Man-

chester Royal Infirmary, hit upon, and which seems to answer every purpose, in being tasteless, transparent, and readily prepared. He dissolves ten grains of phosphorus in two ounces of ether, agitating the solution from time to time; and of this solution, one minim (containing one-hundredth of a grain) is administered in an ounce of water with half a drachm of glycerine. The glycerine suspends the phosphorus so perfectly that a transparent mixture is the result. The addition of a little bitter infusion entirely removes any *soupçon* of lucifer-matches which may hover about the medicine.—*Edinburgh Medical Journal, Dec.*, 1873, *p.* 572.

20.—INJECTION OF CHLORAL IN TETANUS.

M. Bouillaud, on the part of M. Oré, a professor of the Bordeaux Medical School, related a case to the Académie des Sciences, February 16, in which chloral had been injected into the veins as a remedy in traumatic tetanus. A man after a wound of his finger became the subject of tetanus, in consequence of which his mouth became so closed that no remedy could be administered. M. Oré therefore threw an injection containing ten grammes of chloral into the veins, which produced peaceful sleep; and this was followed by a second and third injection, with the effect of obtaining a sleep of eight hours.—*L'Institut*, Feb. 18. [The patient was going on well when the above statement was made, which might have been postponed until the issue of the case had been decided.]—*Medical Times and Gazette, Feb.* 28, 1874. *p.* 254.

DISEASES OF THE ORGANS OF RESPIRATION.

21.—ON "REST" IN THE TREATMENT OF CHEST AFFECTIONS.

By Dr. FREDERICK T. ROBERTS, Assistant Physician to University College Hospital, and to the Brompton Consumptive Hospital.

[There is no doubt that tissues actually diseased or in danger of becoming so, require to some extent mechanical rest, just as does a diseased joint. This remark applies to all chest affections, but in the present article its bearing on pleurisy only is considered.]

To anyone who is acquainted with the pathological conditions resulting from this disease, it must be evident that to restrain

the movements of the affected structure, to prevent the friction of the inflamed surfaces against each other, and keep the parts as much at rest as possible, ought to be the *first principle* to be followed in its treatment. The state of things is very similar to that observed in connection with an inflamed synovial membrane in a joint, and in this condition the first thing the surgeon attends to is to maintain the joint perfectly motionless. I have never seen this principle definitely advocated for the treatment of pleurisy in the published writings on this affection, and, with the exception of two or three suggestions on the subject, it does not seem to have received any particular notice. In the course of my hospital practice during the last ten years, a large number of cases of this complaint have, as a matter of course, come under my care. In the year 1864 I, quite independently, arrived at the conclusion, from certain observations, that the primary object to be arrived at in treating pleurisy should be to endeavour to procure rest; and since that time experience has fully convinced me that I was not mistaken in my opinion. I have on previous occasions brought the subject before the profession, among others in a paper on "Rest and Position in the treatment of Medical Diseases," published in vol. ii. of the "Liverpool Medical and Surgical Reports," 1868. In my "Handbook of Medicine," lately published, the principle has been definitely laid down and advocated, and an indication given of the mode which I usually adopt in order to procure the needed rest. Now I propose to present a more detailed account of my experience of this treatment, and of the manner in which it is carried out.

With regard to the methods employed for procuring rest in the treatment of pleurisy, that which is of special importance is the use of certain appliances fixed round the affected side more or less extensively, so as to limit or prevent its movements. As subordinate aids, it is advantageous in severe cases to keep the patient quiet; to give instructions to restrain the breathing as much as possible (though this is generally done instinctively, on account of the pain the act induces); and to forbid all conversation. In a pathological point of view the results which might be fairly anticipated from this mode of treatment are, that the inflammation would be limited and subdued; that the effusion of lymph and fluid would be checked; and that whatever morbid exudations had been poured out would be more readily absorbed, followed by organisation of the remaining lymph, with the formation of adhesions.

In actual practice cases of pleurisy present very considerable differences when they come under observation, and it will be necessary to point out to what extent the treatment by rest is applicable to the different classes. In one group of cases, of

very common occurrence in hospital practice, the inflammation is localised to a small patch, and appears to have but little tendency to spread. There is generally a good deal of pain, especially on breathing or coughing, but there are no constitutional symptoms. Physical examination reveals limited friction-sound. In these instances the firm application of three or four strips of plaster round the side, in the manner to be presently described, almost invariably gives complete relief, and even allows the patient to continue his occupation. Nothing more is needed, and in a week or two the plasters may be removed. In another class of cases, a patient comes under notice who is evidently in the early stage of a severe and extensive attack of pleurisy, judging by the local and general symptoms and physical signs. It is an unfortunate fact, however, that in many instances the symptoms are not very prominent at first; and it is by no means uncommon to find that abundant effusion has taken place before the patient is aware that there is anything particularly wrong. Should a case come under treatment in this early stage, I would strongly recommend that a trial should be given to the plan of *mechanically fixing the entire side* by one of the methods to be now described. In order to be of any use it should be done effectually, so as to restrain the movements as much as possible, and the sooner the application is made, the more likely is it to be of service. The plan I originally adopted was the following:—Strips of adhesive plaster, from four to five inches wide, were fixed at one end close to the spine, and then drawn tightly round the side as far as the middle line in front, the patient being directed to expire deeply. In this manner the whole side was included, commencing from below and proceeding upwards, each succeeding strip partially overlapping the one below. One was also fixed over the shoulder. Over this layer of plaster strips of bandage of the same width were fixed in like manner, having been previously dipped in a mixture of mucilage and chalk, such as is used in the treatment of fractures. Two or three layers of these were laid on, and then heated sand-bags applied, in order to dry the application as soon as possible. This is a most effectual mode of fixing one side of the chest, while it leaves the other quite free to act; and I would, by the way, commend it to those who are called upon to treat fractured ribs. The plaster adheres firmly to the skin, and the bandages adhere to the plaster, a firm casing being formed which will remain on any length of time. With regard to pleurisy, however, I have since then adopted another plan, which, so far as this disease is concerned, seems sufficiently efficacious. It is merely to use strips of plaster, putting on two or three layers in the following manner:—The first strip is laid on obliquely

in the direction of the ribs, the second *across the course of the ribs*, the third in the direction of the first, about half overlapping it, the fourth as the second, and so on until the entire side is covered. A strip is also passed over the shoulder, which is kept down by another fixed round the side across its ends. Now it is difficult positively to prove that this treatment actually checks the course of pleurisy; but, taking a common-sense view of the matter, it is not improbable that such a result might be anticipated; and, from my own experience, I have not the slightest doubt but that it is brought about. I have carried it out now in a good number of cases, and in all the course and termination have been most satisfactory, while relief to the pain and other distressing symptoms has been generally immediate. I feel convinced, also, that in many of those cases of extensive pleuritic effusion which come under observation, the accumulation might have been prevented or moderated had this plan of treatment been adopted at an early period.

In another set of cases of pleurisy there is found to be a moderate amount of effusion when the patient first comes under treatment. Here, too, I would recommend efficient fixing of the side. In those cases in which I have carried it out, I have almost always had satisfactory results, whereas I have more than once regretted the neglect of this plan of treatment. Where the effusion is very abundant, but little can be expected from it, though I think that occasionally it has appeared to aid absorption. Now and then cases present themselves in which there is extensive exudation of lymph over the surfaces of the pleura, with but little fluid. Here the only object to be desired is to bring about adhesion of the surfaces as soon as possible, and strapping the side firmly aids this most certainly. In bilateral pleurisy of course this mode of treatment can scarcely be practised; and where this disease complicates others it will be of less service.—*Practitioner, March*, 1874, *p.* 182.

DISEASES OF THE ORGANS OF DIGESTION.

22.—ON THE SOLVENT ACTION OF PAPYA JUICE ON THE NITROGENOUS ARTICLES OF FOOD.

By Assistant-Surgeon Dr. G. C. Roy, Corresponding Member of the Glasgow Medico-Chirurgical Society.

[Popular beliefs are often pregnant of wholesome truths. It was on the evidence of an ignorant body of milkmen that Dr. Jenner brought to light the protective influence of cow-pox in the human system.]

It is the practice amongst the native cooks in India to add a few drops of the milky juice of the plant under consideration to tough old meat, to make it tender and supple. Four years ago, anxious to ascertain whether any such virtue really existed in the plant, I added a few drops of the fresh juice to a pound of minced goat's meat, and stewed it over a slow fire. To my surprise, the whole ran into a diffluent mass in five minutes, owing to a larger quantity of the juice having been used on the occasion than is necessary to make it tender and eatable. Since then I had no further opportunity to test its property until I went to England, when I carried with me a quantity of the juice obtained by incising superficially the unripe fruit, and drying it in the sun. Through the kindness of Dr. Parkes, the esteemed professor of Netley, I was allowed the opportunity to carry on the experiment.

The plant belongs to the natural order Papayacæ, and is termed Carica Papya. It is indigenous to tropical climate, and has a rapid growth, consisting of an unbranched stem from ten to twelve feet high, surmounted by large, deeply palmated stalked leaves. The fruits or pepos are edible both in their ripe and unripe state. The former is used as dessert, and the latter cooked as vegetable. The fruit is about the size of a melon, with a green rind, enclosing a sweet, delicious pulp, in which innumerable seeds, about the size and appearance of black pepper, are imbedded. The plant grows extensively in Bengal and over all India, and bears fruit in one year.

Incisions on the stem, or breaking off the leaf from its joint, yield a few drops of this milky juice, but an abundant flow of it can be obtained by scarifying the unripe fruit in the same way as the poppy capsules are treated. The fluid is liable to decompose, especially in hot weather, if kept over for a few days, and hence it should be dried in the sun in shallow dishes, and stored for use. One ounce of the juice can be obtained in an hour's time from the fruits of a vigorous plant. The dried stuff is of a yellowish white colour, hard, friable, and has a peculiar smell. Two drams of this beaten up with one ounce of water will, I believe, give the approximate proportion of strength of the original liquid. At the risk of being tedious, I will give the details of the experiment as they were noted down at the time.

April 30th—thermometer 66 deg. The juice was obtained on 20th December, *en route* to England. A solution of it was made of the strength of 1 gramme to 3 c. c. of distilled water. Two pieces of fresh lean of beef, each weighing ten grammes, were taken and finely minced. Whilst to the one 10 c. c. of distilled water were simply added, to the other 1 c. c. of the made-up solution was mixed up with 9 c. c. of distilled water

and added also. Both cups boiled for five minutes. The medicated portion became soft and half dissolved in the fluid, whilst the other dish remained tough and unchanged.

Another piece of the same meat, of the same weight, was moistened in lump with 1 c. c. of the solution, and kept over for next day's observation.

1st May. The superficial portion of the lump was soft and gelatinous, making the piece very slippery to the touch. When cut up into pieces, and a little more water added, the whole became pulpy in an hour's time, without the aid of heat.

The animal was killed on the 28th April, the thermometer standing at 66 deg.

Another experiment which was made the same day to confirm the result was equally successful.

Two of the cups, medicated and unmedicated, were kept under cover by Dr. Parkes, to note whether the solution had any power to retard decomposition.

2nd May. Decomposition had commenced in both the vessels, but it was more advanced in the cup with papya solution.

27th May. A fresh solution was made (1 gramme to 20 c. c. of water), and the following experiments were tried:—

In four separate dishes, beef, hard boiled white of egg, freshly prepared moist, gluten and arrow root, each 10 grammes in weight, were treated with 2 c. c. of the solution by 8 c. c. of distilled water. To make a standard for comparison, the same articles, of similar weight, were digested in four more dishes, with 10 c. c. of distilled water only. The whole kept over till next day without heat.

28th May. The dish of meat with papya soon became gelatinous on being stirred with a glass rod. The albumen of egg was soft, and when mashed, broke into a uniform pulp. The gluten specimen was soft on the surface, and its superficial layer dissolved in the fluid, giving it a slight turbidity. Arrow-root was dry and visibly unchanged.

The dishes with water were unaffected. The meat fibres remained without change. The egg, when broken up, remained lumpy. The gluten was clear, and even when it was worked up with finger, did not give the liquid the least turbidity. Arrow-root dish was moist, but without any change.

It should be remarked that the dish of meat and of albumen contained more fluid than the corresponding ones with water, whilst the arrow-root dish, which was unaffected in both instances, was dry in one and moist in the other.

All the dishes were preserved for further test.

29th May. Both the dishes of meat were eaten away by a cat, which grew exceedingly fond of it. The whole of the

gluten was dissolved in the solution, whilst the dish with water was yet unchanged. Each of the cups was mixed up with 50 c. c. of distilled water, and left over for next day.

30th May. The cups filtered for test. The two arrow-root solutions gave no reaction when tested with Fehling's Solution for sugar.

The drugged albumen was fast undergoing decomposition, and had an offensive smell. When filtered, nearly the whole of it passed through. The filtrate was clear and decidedly acid. It gave no precipitate with heat, or heat and nitric acid, or ferrocyanate of potash.

The watery specimen of albumen, when filtered, left behind a quantity of residue. The filtrate was milky in colour and neutral in reaction, and gave a slight precipitate with each of the reagents.

The specimen of gluten solution in papya passed through the filter, leaving merely a trace behind. The solution was tolerably clear and acid, and gave an abundant precipitate with heat. More than ¼ was coagulated with heat and nitric acid, and some flocculi formed with ferrocyanate of potash. The precipitate, with heat, was insoluble in liqr. potassæ.

In the watery dish of gluten very little was dissolved. The lump was still sticky. The water solution was clear and strongly acid, and gave no precipitate with any of the reagents. Nitric acid made the solution clearer than before.

3rd June. Solution of the strength of ½ gramme to 10 c. c. of water. As this was kept over for some days before use, it was found to have lost its acid reaction, and settled itself into two parts—the clear liquid above, and a sediment below. The clear supernatant liquid gave the same reaction with a piece of meat, showing that the solvent agent was soluble. A standard preparation of meat with water was made at the same time.

5th June. Both the dishes were mixed with 50 c. c. of water, and left to filter through.

8th June. The papya dish was somewhat advanced, whilst in the watery dish there was no sign of putrefaction. 10 c. c. of the papya filtrate was dried in a porcelain crucible in a hot air bath, and 10 c. c. of the watery filtrate was treated in a similar way. The quantity of solid dissolved out in the papya filtrate weighed 2 grammes, whilst in the watery filtrate it was ·09 grammes.

Another equal quantity of each filtrate was tested in a test tube with heat and nitric acid, and it confirmed the result that the albumen dissolved out in one was more than double the quantity in the other.

A piece of gelatinous meat, as changed by papya juice, was

examined next day by Dr. Welsch, under the microscope. It swarmed with vibriones in active motion. As for the muscular fibres, the disintegration was complete, and those fasciculi that were yet entire were just separating themselves into ultimate particles. The digestion could not have been more complete.

As very little solid stuff was left for further research, the remnant was reserved for chemical analysis.

A solution of the strength of 1 gramme to 30 c. c. of distilled water was prepared and filtered. Reaction distinctly acid. A portion was boiled to dryness in a crucible. The vapour did not redden litmus, but the concentrated solution became more strongly acid, and remained so when thoroughly dried. A little more heating charred the side of the capsule. When incinerated the ash gave an alkaline reaction. A deep precipitate on boiling. The coagulum was strained, and a portion treated with the following reagents. Nitrate of silver gave a white precipitate soluble in ammonia and acids; no precipitate with chloride of calcium, cold or boiled; no change of colour with perchloride of iron; a white cloudy precipitate with liqr. potassæ; some precipitate with chloride of barium.

Remarks.—The above experiments in detail conclusively show the solvent action of the juice on all nitrogenous articles of food. Its effect in making the meat tender has been noticed in several botanical works, East and West Indian, but, so far as I have been able to find, no systematic experiment has been made up to the present to test its virtues medicinally. Some have contented themselves in merely mentioning the practice of the natives as alluded to above, and some West Indian authors ascribe to the plant the power of hastening decomposition in dead animals—so much so that they go the length to assert that a joint suspended under its branches will fall to pieces when cooked. Nay, the belief in the West Indies is so strong, in its power of hastening decomposition, that it is said that live animals fed on the unripe fruit will not keep long after death.

That there is a considerable power of disintegration inherent in the plant is established beyond doubt and cavil. But the question is, what is its peculiar nature? Is it chemical or dynamical? Is it, like the yeast, a fermenting agent, the presence of which in dead animal substances destroys the stability of their composition; or, are the solution and disintegration allied to natural digestion, and the results of chemical change? If the former, there is a valuable discovery of a nitrogenous ferment, which stands in the same relation to protein compounds as yeast does to starch. The conversion of insoluble starch to soluble substances constitutes the process of digestion of amylaceous principles of food in our mouth, and

this conversion is chiefly assisted by an animal ferment, Ptyaline, that exists in saliva. The digestion of nitrogenous principles is mostly a chemical process, in which the gastric juice plays an important part. The rapidity and completeness with which the papya juice acts on meat, when aided by high temperature, surpasses all digestive processes on record. The smallness of quantity used to bring about the change negatives the assumption of any caustic virtue in the plant. Besides, I have put the juice on my tongue, and applied it to the skin without any irritant effect. I was at first inclined to believe the solvent action as due to some fixed organic acid, either tartaric, citric or malic, as will be seen from the records of analysis, but I have failed to arrive at any determined result. The disintegration takes place too soon to be the effect of mere putrefaction. The moving vibriones observed under the microscope were no doubt generated by keeping, and were not the cause, but the effect of disintegration. In all putrefactive changes these are looked upon as the initiators. But inasmuch as the boiling temperature which destroys the existence of vibriones hastens this peculiar change, it is fair to suppose that the solvent action is something different from putrefaction. Besides, no reagent has yet been able to bring about putrefaction in fresh meat, in five minutes. The fruits in their ripe and unripe state are edible and quite harmless.

The digestive agent is not acid, for its reaction is too feeble, and even when long keeping makes it ammoniacal and neutralizes the acidity, it yet retains its peculiar virtue. The solvent principle is soluble in water. Coagulated albumen dissolved by it will not coagulate again with heat acquiring the property of albuminose; gluten is thoroughly dissolved, and can be re-precipitated.

The whole action is so identical to healthy digestion that I wonder we have not availed ourselves of this medicinal property, in cases of invalids and dyspeptics, to substitute a process of artificial digestion.

A few grains taken immediately after a meal will substitute the power where it is wanting. I have not tried it in any case internally, but from its effect on the cat, which grew so fond of it that it became a task to prevent its depredation, I believe it is harmless. My attention was also directed to discover whether it could be utilized in preparing soluble meat or something like a liquid extract, but its liability to decomposition is a bar to its use. Further investigations are yet wanting to establish its repute, but, as far as they have been gone into, the result is highly encouraging. I intend to resume my operations as soon as I am relieved of my present arduous work, which leaves no time for experiment.—*Glasgow Med. Journal, Jan.*, 1874, *p.* 33.

23.—ON CARLSBAD AND ITS MINERAL SPRINGS IN RELATION TO AFFECTIONS OF THE LIVER.

By Dr. STEPHEN H. WARD, Visiting Physician to the Seamen's Hospital, London.

Anyone who desires to study the physiognomy or, as I would rather express it, the external aspect of diseases and constitutional affections in their more chronic form should visit the different continental watering-places during the height of the season. Dr. James Johnson glances at the characteristic aspects of disease which distinguish different health-resorts, as Spa, Schwalbach, Wisbaden, Wildbad, Kissingen, &c.; but he considers that Carlsbad presents a greater medley than any of them. However, one will not have been long at the last-named spa without discovering the prevalence of the sallow or more or less jaundiced complexion, pointing to liver affections as those for which it is largely resorted to.

The limits of my paper will not allow me to do more than bestow a short notice on the different hepatic disorders which are likely to be benefited by the Carlsbad "cure." They are—

1. *Hyperæmia*, indicated by a sense of fulness and uneasiness in the right hypochondriac and epigastric regions, by more or less dyspepsia, by sallowness or even a jaundiced tint of complexion, and generally, at first, by an undue secretion of bile, which may show itself in bilious vomiting and diarrhœa. The first stage will, in most cases, have been treated at home, and the hyperæmia will have merged into more or less chronic congestion, with enlargement of liver, perverted or deficient secretion, &c., before the patient from England gets to Carlsbad. Immoderate eating and drinking, and especially habitual indulgence in rich articles of food, and the lethargy and disinclination for exertion induced by the habits of the gourmand, are the usual exciting causes of hyperæmia of the liver, and engorgement of the vessels of other abdominal viscera. Direct irritation and congestion of the liver occur also as the result of undue and frequent use of alcoholic liquors, especially upon the empty stomach, such congestion causing more or less enlargement of the viscus, and being often but the first stage of cirrhosis.

2. *Fatty liver* (infiltration, not degeneration) is usually but a symptom of a general deposition of fat in and about the tissues and organs of the body; and when it is the result of the excessive indulgence alluded to under the previous head, coupled with deficient exercise, will probably be much relieved by the Carlsbad "cure." It is only in an advanced stage of this affection that we have any marked symptoms, as enlargement of the liver, uneasiness and dragging sensation on lying on the left side,

irregular and indolent functional action, annoying flatulence, a sallow and often a greasy state of skin. There is something in the rapid decrease of fat under these mineral waters which is more than can be explained by the rigid adoption of altered hygienic arrangements, and may be fairly attributed in some measure to the action of the waters themselves. Should the state of the pulse, &c., indicate fatty accumulations about the heart, or should irregularity in the circulation, tendency to syncope or vertigo, point to the possibility of degeneration of tissue, the "cure" will be contra-indicated, or have to be adopted with much caution.

3. *Jaundice*, where it is a symptom of the preceding affections, or associated with chronic catarrh of the gall-ducts, or with habitual obstruction of the bowels and accumulations in the large intestines, will be relieved by the "cure." As Niemeyer, however, remarks, "if jaundiced patients with an incurable obstruction of the bile ducts go to Carlsbad, their jaundice is not improved by the use of the waters; but they die sooner than they otherwise would, because the symptoms of congestion are increased, and the destruction of the liver-cells is hastened by augmented secretion."

4. *Gall-stones and biliary concretions.*—For these the Carlsbad "waters" are considered almost a specific. They remove those already formed, have, indeed, the credit, which must be taken *cum grano salis*, of a solvent action, and act as a preventive of re-formation. Gall-stones would appear, according to the experience of Carlsbad physicians, to be far more common than they have been considered to be. Out of 372 patients who came under the care of Dr. Hlawacek in one year, 64 had gall-stones, and out of 581 patients in another year, 91. The fact is that gall-stones are sought after at Carlsbad very carefully, by washing the evacuations over a fine sieve, and many small biliary concretions are thus found which have painlessly passed the ducts into the duodenum, and which would have escaped the notice of ordinary medical observers.

Chronic derangement of the functions of the liver, usually torpor, with or without enlargement of the organ, the result of climatic influences in India or of exposure to malaria, especially when associated with much cachexia and anæmia, is not likely to be benefited much by Carlsbad and its waters. Here the more tonic waters of other spas will be indicated, or the occasional use of saline aperients, iron and quinine, and the nitro-muriatic acid treatment.

Individuals suffering from congestion of liver secondary to advanced lung or heart mischief, or from cancerous affection or amyloid or fatty degeneration of the liver, or from cirrhosis in

its advanced stage, had better remain at home under the treatment of their own medical advisers.

The Carlsbad cure is a powerful remedy, and in properly selected cases is doubtless very valuable. Many individuals, however, get worse under it, and are consoled by the statement that they will feel the benefit when they return home; and this may prove true in many instances. But to some the benefit does not come, and the inevitable termination of serious organic mischief has been precipitated, or an unfavourable result has been determined, in affections which, under different treatment, might have done well. I am not saying this to discourage or deter individuals who propose seeking renewal of health, at Carlsbad or elsewhere, but simply to show how important it is that an invalid's case and constitutional power should be well considered before he starts for a goal some 700 or 800 miles distant, where, after all, the desired end may not be attained.

What is called the "nachcur," the after-cure, is not less essential than the "vorcur." The local physicians recommend that the patient, after he has returned home, or gone elsewhere from Carlsbad, should take the bottled mineral water in moderate quantity, and persevere also in following the hygienic rules which were enforced during the "cure;" but the "nachcur," in its larger sense, should extend over the remainder of an individual's life, and should consist in strict avoidance of the habits which led to impaired health.

The Carlsbad season lasts from May to the end of September, and the period of cure for each invalid ranges from two or three to eight or nine weeks. Those who are wanting in vigour, or not tolerant of heat, will do well to select May and June or September for their sojourn at Carlsbad. If means and time permit absence during three or four months, it will be a good plan to get the "cure" over in the spring and early summer, and afterwards to get braced in some mountain district. For some cases the warmer months, July and August, will be suitable.

In conclusion, I would remark that a large number of invalids go to Carlsbad and other watering places who may derive but little benefit from the morning draughts from the springs, but who yet, in strict attention to health-laws, as regards diet, exercise, rest, and recreation, in fresh scenes and associations, in the escape from the anxieties and duties of every day-life, may succeed in finding the *basilisk*, the search after which forms the subject of an amusing and instructive episode in Voltaire's admirable tale "Zadig."—*Lancet, Aug.* 2, 1873, *p.* 145.

24.—OXIDE OF ZINC IN THE TREATMENT OF DIARRHŒA.

By Dr. WILLIAM BERRY, House Surgeon to the Lancaster Infirmary.

[This remedy was first recommended in diarrhœa by Dr. Brakenridge, of Edinburgh.]

I have every reason to be satisfied with it as a remedy for diarrhœa in children, especially in those in which the cause appears to be some irritation of the nerve-centres presiding over the alimentary canal. In adults I have found it useful in cases of lienteric diarrhœa, but not so beneficial as the aromatic chalk powder of the pharmacopœia in ordinary cases.

I think with Dr. Brakenridge that in the majority of cases of diarrhœa in children—though not in all—the nervous system plays an important part; especially in those children who are teething, and in children of the poorer classes who are ill fed and badly clothed.

Whether we accept the theory of Dr. Brakenridge as to the cause of diarrhœa in children or not, there can be no doubt that the beneficial effects derived from the remedy are due to its tonic and astringent properties, I am inclined for my own part to think that its antispasmodic properties have little to do with its efficacy.

That infantile diarrhœa is in a great measure due to a debilitated state of the nervous system and to a hyperæmic condition of the mucous membrane of the bowels, is proved by the readiness with which it is relieved by oxide of zinc.

In the diarrhœa of teething children, and those whose digestion is at fault, the frequency of the evacuations is at once checked, and the character of the motions altered.

The remedy produces nausea in some cases unless a little food be administered just before it, but in many cases no nausea is produced although this point is not attended to.

[Some cases are then related, and we take from these the following prescription used in the case of a child one year old:—℞. Zinci oxidi gr. 32; mucilaginis; syrup. simpl. āā ʒ ij); aquæ ad ℥ij. S. ʒ j 3tis horis.]—*Practitioner, Nov.* 1873, *p.* 356.

25.—FÆCAL ACCUMULATION.

By Dr. THOMAS COLE, Physician to the Royal United Hospital, Bath.

Mr. S., aged 60, had been more or less ill for two years, with gradually increasing distension of the abdomen, accompanied by some emaciation, loss of appetite, much flatus, and confined

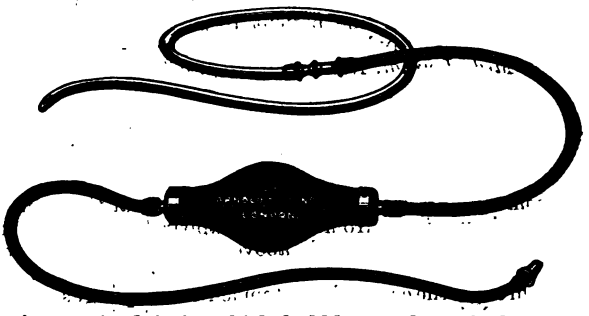
bowels. On examination, I detected an immense tumour filling the whole of the abdomen, except a small portion of the left lumbar region, where there was tympanitis. It passed upwards under the ribs, downwards into the pelvis, was perfectly smooth, very hard, and quite immovable; over a small part, to the right of the umbilicus, was indistinct resonance; and somewhat below this, I felt a coil of small intestine between the tumour and abdominal wall, evidently filled with fæcal matter. By firmly pressing the finger against the swelling, at different parts, I found that a slight indentation was made, though with difficulty. The superficial abdominal veins were much distended. The rectum was full of large hard lumps. My diagnosis was fæcal accumulation to an extent I never saw nor heard of before. Large injections of warm water daily for three weeks, aided by small doses of belladonna and nux vomica, brought away a most astonishing quantity of fæcal matter, and resulted in the total disappearance of the tumour, and the restoration of the patient to perfect health. I may add, that homœopathy had had a lengthened trial in this case. —*British Med. Journal, Nov.* 29, 1873, *p.* 630.

26.—AN IMPROVED SYPHON STOMACH-PUMP.

By Dr. Louis H. Tosswill, Exeter.

For some time past I have felt convinced that the stomach-pump now in use is far from a perfect apparatus. It is costly, not particularly portable, and, above all, a dangerous instrument; cases being known where the stomach-tube has been pushed through the coats of the stomach or œsophagus. I wish therefore to bring before the notice of the profession a stomach-pump on a novel principle, which has been made for me by Messrs. Arnold and Sons, the well-known instrument makers in West Smithfield. The advantages I claim for my instrument are —freedom from danger, cheapness, and increased portability. It can be easily carried in the coat-pocket, its cost does not exceed 15s., and it is almost impossible to perforate the coats of the stomach or œsophagus by means of it, be the operator ever so clumsy or ignorant. The instrument consists of an indiarubber tube, about half an inch in diameter and some four feet in length, provided with a ball of the same material, which is three or four inches in diameter, and situated about eighteen inches from one end of it. In fact, it is almost a *fac-simile* of one of Higginson's enema syringes, but without a valve. To the shorter length of tube the stomach-tube proper is attached by a simple contrivance. This stomach-tube is nothing more nor less than an enlarged Thompson's indiarubber catheter with two large oval

holes situated near the extremity. Let us suppose the instrument to be used in a case of poisoning where it is desired to wash out the stomach as speedily as possible. The patient being seated in a chair or reclining on a couch, the stomach-tube is oiled and then passed back in the mouth in the usual way until it has reached the upper part of the pharynx. Its passage down the œsophagus, as in the case of Thompson's catheter in the urethra, is effected by twisting it round and round in the hand until it has reached the stomach. I need scarcely point out how much less pain this soft flexible tube must cause than the hard, somewhat inelastic tube generally used. The rest of the instrument is now adjusted to the stomach-tube (the work of a second), and then a jug of water being obtained, the end of the tube beyond the ball is introduced into it. The operator with one hand pinching the indiarubber tube somewhere between the ball and the patient's mouth, with the other hand compresses the elastic ball, and thus forces out some of



the air contained in it, which bubbles up through the water in the jug. When this has been repeated two or three times all the air is forced out, and the ball becomes filled with water. If the jug be now raised a foot or two above the patient's stomach, and the pinching of the tube discontinued, the instrument acts at once as a syphon, and the water flows from the jug in a continuous stream into the patient's stomach. When enough has been introduced, the tube is pinched somewhere between the ball and the patient's mouth, and the flow of water into the stomach is at once stopped; the water being retained in the ball by the atmospheric pressure. If the end of the tube be now taken out of the jug, and put into a basin below the level of the patient's stomach, upon the pinching of the tube being discontinued, the contents of the stomach will at once flow out into the basin in a continuous stream, the instrument acting of course again as a

syphon, only in the reverse way to that previously. When it is believed that the stomach is nearly empty, the tube is pinched between the ball and the basin, the end of the tube put back into the jug, and the whole thing repeated over again. The object of pinching the tube when the end of it is being shifted from the jug to the basin, and *vice versâ*, is of course to retain the water in it by means of the atmospheric pressure, thus enabling it to act as a syphon. In a case of opium poisoning I have lately used this instrument, and by means of it thoroughly washed out the patient's stomach in a very brief space of time. In case the holes at the end of the stomach-tube become clogged with food, they can be easily freed by pinching the tube between the ball and the jug; compression of the ball will then force out the fluid contained in it, and thus clear the holes. In conclusion, I would draw special attention to the low price at which this instrument can be obtained. I do not think I am wrong in asserting that the great majority of general practitioners do not possess a stomach-pump. This is chiefly due, I believe, to the high price of the instrument hitherto used. This objection being now removed, there can no longer be an excuse for any practitioner being unprovided with an instrument, the want of which may materially tend to lessen the chances of recovery in a case of poisoning.—*Medical Times and Gazette, Feb.* 14, 1874, *p.* 199.

27.—ON THE TREATMENT OF THREADWORM.

By Dr. T. Spencer Cobbold, Lecturer on Parasitic Diseases at the Middlesex Hospital.

[We cannot expect in the curative treatment of threadworm a success equal to that which ought to be obtained in the case of tapeworm. The worst of cases, however, may be overcome by perseverance in the application of appropriate remedies in combination with the employment of hygienic measures.]

I have ransacked the Pharmacopœia for permanently effective remedies, but I have satisfied myself that no single drug, or any combination of drugs, can be employed with any certainty of perfect success. You cannot find any remedial agent that exerts what may be called a specially poisonous or specific action upon the threadworm. However, in the case of the large roundworms, that is, the ascarides properly so called, the action of santonine is almost that of a genuine specific. It is, indeed, a valuable lumbricifuge; but, as an oxyurifuge, I confess I am at a loss to understand why its action is so much extolled abroad. I have used it extensively, and can only report

unfavourably of its action in oxyurides. On the other hand, all the larger kinds of nematode worms are readily poisoned by it.

As to the time-honoured employment of injections of infusion of quassia, or of lime-water enemata, or of others containing salt, castor-oil, santonine, and what not besides, all that can be said of them, in the case of adults, is, that they undoubtedly afford partial relief; but, in view of contributing to a permanent cure, they avail nothing. Even the employment of long elastic syringe-tubes will not enable patients to reach the true seat of the helminths; and one cannot but perceive the inconvenience, to say nothing of the danger, of recommending their use. I see by a statement, reproduced in a recent number of the London Medical Record, that Professor Mosler recommends the employment of large enemata for the treatment of entozoa in general, and for the expulsion of oxyurides in particular. In the case of imperfectly expelled tapeworms (which he believes to possess the power of anchoring themselves afresh in the colon after expulsion from the small intestine), he even recommends the use of milk-and-water enemata up to the amount of three pints. I will only digress so far from the matter immediately before us, as to say that I have repeatedly expelled heads of tapeworms from the colon without giving injections; and I must say that I cannot bring myself to believe in the supposition of any true re-anchorage occurring in cases where the head of the worm has been dislodged from the smaller bowel. In this connection, you will not have forgotten the case of the young daughter of a physician, where I had only the upper part of the head itself left to operate on, and yet I succeeded in dislodging it. The four suckers were so slightly connected, that they separated from each other in the water of the phial during gentle agitation. The case was one, the treatment of which had previously baffled several experienced medical practitioners.

Notwithstanding recent investigations, and the clearly enunciated conclusions based on them, I find that all our ordinary manuals, handbooks, vade-mecums, and such like generally useful treatises, continue to encourage the old and erroneous notion that the threadworms reside in the rectum and sigmoid flexure of the colon. Thus, even a writer in the Lancet of last year (Feb. 10th, 1872, p. 185), says:—"As oxyures infest the lower bowel, they are best reached by enemata." Of course, if this were the whole truth, the injections of quassia and salt, recommended by Mr. Date, would alone suffice to get rid of the parasites. The fact is, however, that the entire length of the colon is the territory inhabited by the threadworm, the cæcum itself constituting the parasites'

true head-quarters. Knowing this, therefore, to be the case, and likewise understanding, as we now do, how these entozoa originally gain access to their human bearers, it is clear that we are in a position to treat the threadworm helminthiasis on thoroughly rational principles. Injections, as I have said, cannot be conveniently employed to wash out the contents of the arch of the colon and cæcum, and therefore we must, in the main, rely upon the operation of medicines taken by the mouth. From the very first, I have insisted upon this mode of procedure, employing injections only for the purpose of dislodging such oxyurides as have been driven down to the lower bowel. When it comes to the question of the choice of drugs, you will find that a great variety of medicinal agents may be used with good effect. Some seem to answer better in particular cases than in others. Speaking generally, I give a preference to active saline cathartics repeated for several days in succession, followed by the use of cold water enemata. Small doses of chloric ether and sulphate of iron are eminently serviceable additions, and the same may be said of aloes, with or without assafœtida. In cases where these drugs are objected to, I have employed various active mineral waters with good results. Bitters of all kinds are useful, and patients who object to salines will swallow any reasonable amount of the infusion of gentian and other pure vegetable tonics. As a rule, oxyurifuges should be administered in the form of copious draughts, and the oftener they are repeated, for a short interval, the more effective will they prove in the end. The rapid passage of the drugs through the bowel, will, if several times repeated, carry most of the parasites sufficiently low down to be within reach of the clysters; and the prevention of the return of the parasites to the upper part of the colon is one of the practical points worth looking to. The employment, therefore, of enemata, after the exhibition of remedies by the mouth, cannot fail to prove beneficial.

I am free to admit that, do what we will, some cases prove obstinate and apparently incurable. How are you to account for this? Is it really true that the disorder may baffle your best efforts? In one sense it may be that you can never succeed; in another sense, you may have been successful without knowing it. What is the probable explanation? I think it is as follows. Real cures are oftener effected than is commonly supposed. Thus, many patients will say that they have not seen a threadworm for the space of six weeks, or even for three months, when, all of a sudden, their enemies return. In these cases, I believe that fresh invasions have taken place, the patients having in the interval swallowed new germs, or more strictly eggs, derived from oxyurides infesting either

themselves or some other persons. Obviously, therefore, if you accept my assurances on this score, it is part and parcel of adequate treatment to recommend such prophylactic measures as shall be likely to reduce the liability of re-infection within the lowest possible limits. Clearly, it avails little to effect a perfect cure, if, within the space of a few weeks, and time after time, the disorder be contracted afresh.—*British Medical Journal, Feb.* 7, 1874, *p.* 167.

DISEASES OF THE URINARY ORGANS.

28.—ALBUMINURIA IN CASES OF VASCULAR BRONCHOCELE AND EXOPHTHALMOS.

By Dr. J. Warburton Begbie, Edinburgh.

[Albuminuria and Bright's disease are not synonymous terms, for there are many cases of the former which are not owing to the existence of the latter. The present paper is intended to draw attention to the presence of albumen in the urine, as a notable feature in some cases of vascular bronchocele and exophthalmos.]

My attention having been called to the association of an albuminous condition of the urine with vascular bronchocele and exophthalmos, in the first instance, owing to the existence, in certain cases, of œdema of the feet and ankles, I became satisfied that the association is by no means of infrequent occurrence. I have found albuminuria in a considerable number of the cases which have lately fallen under observation, and it is more than likely that it may have existed in other instances, although unrecognised from causes to which reference will be made. Albuminuria has existed in persons of both sexes suffering from this disease, and has been more common in cases of female than male patients; but when the much greater frequency of vascular bronchocele and exophthalmos among women than men is kept in view, the symptom has been present in a larger proportion of males than females. In some it has been an evanescent symptom, lasting only for a short time, and when so, only present in limited degree. In others, the albuminuria has been very considerable—it has even been excessive, and it has lasted for weeks, indeed for months—while the other notable symptoms of the complex malady continued, and only disappeared as the latter became relieved or removed. Œdema of the lower limbs, although in the first instance calling attention to the condition of the urine, has not been observed to bear any constant relation to the albuminuria; on the contrary,

œdema, and sometimes considerable anasarca of the legs, have been present without any appearance of albumen in the urine; and albumen, when present, has generally existed without any form of dropsical swelling. In the most notable cases of albuminuria in connection with vascular bronchocele and exophthalmos, dropsy has not been present.

In prosecuting my inquiry on this subject, a very interesting circumstance became manifest, namely, that the albuminuria was in certain cases limited to the period of digestion—present immediately after a meal, and absent when the person fasted. I had in one case been not a little puzzled by noticing the strange variety presented by the urine within very short periods—the albumen present in considerable quantity one day, and absent the next—present in the urine of the forenoon, and not to be detected in that passed before dinner. By obtaining repeatedly specimens of the urine in this case, and in one or two others, I was able to satisfy myself that in this disease the albuminuria is apt to possess the remarkable character of only occurring during or immediately after the digestion of the food.

This appears to me to be a most interesting feature, and it is also in various ways a very important one. The existence of albumen might readily escape detection if the physician relied on the results of one examination, as he is not unapt to do; and even when taking more than usual care, it is quite possible that in several specimens of the urine furnished by the same patient for examination it might be found that no albumen existed. Conceive a patient so affected consulting a medical man before breakfast or luncheon; his urine, carefully examined, found to be non-albuminous; but, from some cause or other—and we know how fickle some patients are—calling for another physician shortly after a meal, when a large precipitate of albumen occurred on applying heat as well as on the addition of a little nitric acid. Remarkable as this character of the albuminuria which occurs in cases of vascular bronchocele and exophthalmos is, there is another feature pertaining to it which is even more striking. The albumen is present in much larger quantity after breakfast than after either luncheon or dinner. In one case of this kind, which I was able to observe at intervals for a considerable time, the urine presented the following characters:—It was passed in average amount, was of healthy colour, reaction, and density. On no occasion was the density observed to fall below 1015, and it never rose above 1025; the average density was that of health, 1020. Very occasionally this urine deposited lithates, and from time to time contained a slight excess of earthy phosphates. Sugar was never present. Albumen existed in this urine daily for upwards of a twelve-month, but only at certain times of the day, and these times

were readily found to be shortly after meals. After breakfast, however, the amount of the albumen was invariably greater than after luncheon, dinner, or an evening meal.

It is surely a satisfactory consideration that a condition of excessive albuminuria—the urine becoming nearly solid on the application of heat and addition of nitric acid—may, after all, not indicate the existence of any structural change in the kidney. Of course, in connection with the albuminuria, the presence or absence of certain other important features must, under such circumstances, be taken into account. Apart from the intermittent character of the coagulability, the facts that the quantity and density and colour of the secretion did not deviate from the healthy standard, and still more, that diligent and repeated examination by the microscope failed to detect the vestige of a cast of any kind, were to be regarded as the proofs of the renal derangement being functional and not organic. Still, there is occasion for reiterating the assurance that albuminuria is not Bright's disease, and for pointing out that, when unconnected with the presence of blood or pus in it, there may be even a highly coagulable condition of the urine, due to causes which are wholly independent of any structural change in the renal substance. Such, I am persuaded, may confidently be affirmed of the albuminuria which is apt to occur in cases of vascular bronchocele and exophthalmos.

Some little time after my attention had been called to the peculiar features of the albuminuria which is incident to cases of vascular bronchocele and exophthalmos, and to which reference has now been briefly made, a very instructive instance of the malady fell under my notice in the person of a medical man, who had been for some years engaged in active practice in the south of England. Calling one day for advice, the gentleman in question told me that he was the subject of Bright's disease, and feared that little or nothing could be done for his relief.

He certainly looked ill, was thin and sallow in appearance, and evidently deeply depressed in spirits. Having, however, noticed, as he entered my room for the first time, that he possessed the prominent eyes, with peculiar staring expression, so characteristic of vascular bronchocele and exophthalmos, I ventured—in reliance on my previous observations—to offer the comforting suggestion, that possibly, if not probably, the presence of albumen in his urine might be due to causes which were capable of being removed, and did not indicate the existence of any serious disease. This remark he received with politeness, but with very evident incredulity, mentioning that his condition had already been condemned by medical authority, that he did not expect to be cured, but only felt justified in expecting a little prolongation of life with greatly impaired

health. On carefully examining this gentleman, a bronchocele of considerable size, soft, and pulsating, of whose existence he had been unaware, was discovered. His pulse was small, and as frequent as 140 per minute. The urine, on its earliest examination, while the patient was fasting, and between 1 and 2 P.M., was to his own surprise, found to be free from albumen, its density 1020, and of acid reaction. Subsequent examinations of the urine determined its decided coagulability after meals, and its freedom from albumen while abstinence from food was practised. This gentleman was exceedingly nervous, and very desponding. Under treatment a considerable improvement took place. As he had occasion to pass through London, I begged him to see Dr. Johnson, and it is to him Dr. Johnson alludes in a letter of date 27th October, 1872, from which I now quote:—"There is a striking resemblance between his case and that of the Scotch clergyman whom you were so good as to send to me. Dr. ——— is extremely feeble and nervous, and I fear that the prognosis is bad. I quite agree with you, that a long sea voyage would be the best course for him, but he seemed unwilling to do anything that would separate him from his family." I have not seen this gentleman for some time, but his progress can be traced in the correspondence I have had with him. On the 28th of March 1873, he wrote:—"I have gone very comfortably through the winter, considering all things. I am much stouter than when you saw me, and much stronger. I do a good deal of walking in the course of the day. My heart still beats very rapidly, but its action is not so irregular as formerly. Whether albuminuria exists, I cannot say, as I never test for it, and try to banish the thought of it altogether from my mind. The goître is decidedly less in size, but is still visible."

I obtained two specimens of urine passed on the 10th of April, 1873. That voided before food was taken, contained deposit of lithates, and was absolutely free from albumen. That passed an hour after breakfast had a density of 1016, and contained a very faint trace of albumen. The letter which accompanied the specimens of urine will exhibit the peculiar nervousness under which my friend still laboured. "I send you," he wrote, "two little bottles with urine which I passed to day. Please do not send me a bad report, as it will only frighten me. I dread the thought of renal disease so much, that I try to banish the very existence of kidneys from my thoughts. That I shall look with anxiety for your next letter, and yet dread its arrival, your knowledge of my nervous condition will assure you." Happily, I had no occasion, to send "a bad report," while, in consideration of his highly sensitive and nervous state, I wrote over the seal of my communication, "good news."

Since the summer of 1873, this gentleman's condition has steadily improved. On the 28th October he wrote:—"I am glad to be able to tell you that my wife was confined on the 27th ultimo, and has done very well indeed. For myself, the only trouble I now have is occasional palpitation." Again, on the 8th December:—"My general health is now very good. I am as fat as ever I was, and my eyes have lost that unnatural stare. There is still a slight enlargement of the thyroid gland, and sometimes—though rarely—my kidneys act very little. Digitalis and iron remedy this. My heart still beats fast." My last communication, of date 17th January, 1874, gave a most satisfactory account of his progress, and was written a few days before he embarked as surgeon of a ship sailing with the royal mails for Madeira, Ascension, and the Cape of Good Hope. Before obtaining this appointment, my friend was obliged to go before the medical officer of the General Post-Office for examination as to personal and professional fitness. In this communication he says:—"I find no remedy relieve me so much as bromide of potassium, in 20-grain doses, thrice daily. Aconite and digitalis do not relieve me much."

Having indicated the nature of the albuminuria which is found in certain cases of vascular bronchocele and exophthalmos, it now becomes necessary to inquire a little more fully into its pathology. Albumen is, in all probability, not a constituent of healthy urine. It has, indeed, been stated by Dr. Gigon of Angoulême, that albumen exists in normal urine, and can be thrown down by chloroform. Becquerel, Aran, and Parkes have satisfied themselves that the precipitate which is produced by the addition of chloroform to the urine is not albumen, but a mixture of chloroform, mucus, and organic substances. Albumen, however, is so frequently present in the urine, and occurs under so vast a variety of circumstances, that it becomes a matter of very great importance to determine its clinical significance. The existence of blood, pus, or spermatic fluid in the urine renders it coagulable; but it is scarcely necessary to state that in the cases of vascular bronchocele and exophthalmos to which I have referred, there were none of these conditions. Again, the excessive use of a diet composed chiefly or entirely of albuminous matter, such as eggs, has been found by various observers—among others, Barreswil, Hammond, and Brown-Séquard—to produce albumen in the urine. Barreswil, after taking ten eggs, passed albuminous urine for twenty-four hours. There can be little doubt that, in some persons peculiarly constituted, the partaking of certain articles of food difficult of digestion by them, produces albuminuria for a time. Of this nature was the case of a student mentioned by Sir Robert Christison, in whom a large amount of cheese or pastry pro-

duced albumen in the urine. Apart, however, from errors of diet, as Dr Parkes has stated, temporary albuminuria will occur in persons with very slight disease. Beneke, when suffering from dyspepsia, noticed albumen in his own urine four times in four weeks. Clemens, Rayer, Martin Solon, and many other physicians, have made similar observations. It may be admitted, then, that albumen, although not an ingredient of healthy urine, may occur in the urine of healthy persons, or of persons whose disorder of health is, at the time of its presence, very slight. Attention has recently been called by Dr. George Johnson to the occurrence of albuminuria in healthy persons after bathing in cold water. Again, albuminuria is apt to occur in relation to a great many disorders which are not essentially connected with structural change in the kidneys. Not to dwell upon pregnancy, as a condition of the system with which albuminuria is associated, there is the puerperal state. Again, there is a large number of febrile and inflammatory diseases in the urine of which albumen very often occurs. Among these may be mentioned scarlet fever, measles, erysipelas, smallpox. diphtheria, typhus and typhoid fever, cholera, &c., and of inflammatory diseases, pneumonia. Over and above these relationships there are various forms of visceral disease—of disease affecting the heart, liver, and lungs—in the urine of which albumen appears; and the physician, in his observation of such cases, is on the outlook for its occurrence, and ascribes it, when it does come, to the general impediment to the circulation which the following diseases—(I name them as illustrative examples only) dilatation of the heart, cirrhosis of the liver, emphysema of the lungs—produce; for, owing to these the renal circulation necessarily suffers. In such diseases, the albuminuria is almost invariably associated with a diminished secretion of urine. It is not necessarily so in the albuminuria of vascular bronchocele and exophthalmos,—the quantity is generally unaffected, and so are the other characters of the urine.

Dr. Roberts of Manchester has very clearly pointed out that in endeavouring to determine whether the presence of albumen in the urine be dependent upon the existence of organic disease of the kidneys or not, the question in each individual case must be considered in connexion with the three following points:—1. The temporary or persistent duration of the albuminuria; 2. The quantity of the albumen present, and the occurrence and character of a deposit of renal derivatives; 3. The presence or absence of any disease outside the kidneys which will account for the albuminuria.

Now, viewing the albuminuria of vascular bronchocele and exophthalmos under these aspects, the following observations

may be made:—1. The albuminura is temporary; for according to Dr. Parkes's definition of that condition, it has totally disappeared while the patient is under observation; but, instead of lasting a few days or weeks, as holds true of most instances of temporary albuminuria, properly so called, it has lasted for many months, indeed for a year. Intermittent or remittent albuminuria would be a better signification than temporary, for the albuminuria of vascular bronchocele and exophthalmos, but, better still, because more definite, albuminuria occurring during or after digestion; 2. The amount of albumen which is present in the urine when the cause of its manifestation is independent of organic or inflammatory disease is usually small; very often it is not more than a mere trace. The amount of albumen in the urine when passive congestion of the kidneys results from cardiac or other visceral disease may indeed be considerable, but I do not remember ever to have seen the urine under such circumstances very highly coagulable; very highly or excessively albuminous has, however, been the character of the urine in at least one example of vascular bronchocele and exophthalmos, the recovery in which has been complete. Again, in the temporary albuminuria of other maladies, there is generally some deviation, often notable, from the normal condition of the urine in other respects; the quantity, density, and reaction of the urine are often affected; and there is the presence of lithates in excess, or an undue amount of earthy phosphates. Not so, generally at least, in the cases of vascular bronchocele and exophthalmos which I have seen. With the exception of its containing albumen in considerable or large amount, the urine has been healthy. Neither has there been in the latter any deposit derived from the kidneys. In this respect, indeed, there is the interesting fact of a copious presence of albumen in the urine, without any trace of casts of one kind or another, and without any renal epithelium or blood. Further, the form of albuminuria which we are now considering, differs from any other form hitherto described, in being limited to the period of digestion of the food. 3. The presence of disease apart from the kidneys is of course conspicuous in vascular bronchocele and exophthalmos, while the peculiar morbid condition of the nervous system and of the bloodvessels in that disease, as well as the spanæmia which exists, must, I think, be taken into consideration in our endeavour to determine the pathology of the albuminuria, which we now know to be in some way or other associated with it.

In vascular bronchocele and exophthalmos, there is always present much disturbance of the nervous system—the sufferers from this disease are invariably highly nervous—they are often

hysterical. The primary disorder of the circulation, both cardiac and vascular, is of the nature which we associate with derangement of the nervous system. The organs and parts of the body in which the local manifestations of disturbance are seated, are organs and parts freely supplied with bloodvessels and blood,—the thyroid gland, the spleen, which, although not invariably, is often affected, and the deep ocular tissues. To these must be added the kidney. From the failure of due nervous influence, the small vessels, and, it may be presumed, the capillaries, in the thyroid gland, and the deep-seated orbital vessels, become dilated, and the circulation through them in consequence interfered with. We can infer from the consideration of the essential nature of the renal circulation that if an obstruction to the return of blood through the inter-tubular capillaries and veins exists, either from an obstruction in the heart or lungs, or from a disordered state of the vessels themselves—a condition which I believe to exist in vascular bronchocele and exophthalmos—favoured by the more or less watery state of the blood itself, there will occur a transudation of serum, carrying with it albumen, through the walls of the Malpighian capillaries into the tubes, and thus the urine will be rendered coagulable.

But in order to explain the limitation of the albuminuria to the period during and after digestion of the food, it is necessary to regard the increased afflux of blood which then takes place, as leading to an altered physical relation between the blood and the walls of the vessels, and likewise determining an engorgement of the Malpighian capillaries, while the loss of tonicity in the different vessels is thus rendered temporarily more injurious. In other words, the renal circulation, in its comparatively tranquil condition, is unaffected by the disordered state of the capillaries and small vessels; but, when excited by the stimulus of a recent meal, it is unequal to the task, and the resulting interference determines the albuminuria.

But, further, there is something in the character of this albuminuria to ally it with the albuminuria of indigestion, to the occurrence of which a brief reference has been made. Sufferers from vascular bronchocele and exophthalmos have frequently an inordinate appetite and craving for food. They have bulimia; and, in the case I have shortly recorded, this symptom was notably present. A large meal taken hurriedly is not unlikely to influence, as a remote cause, the production of the albuminuria. I think Dr. Johnson's explanation of the greater amount of albumen in the urine after breakfast than dinner, may be correct—the same explanation had occurred to my own mind—that the earlier meal taken after a long fast gets

into the blood vessels quickly, and in consequence leads to a greater disturbance of the renal circulation. Besides, hot tea and coffee, with eggs, consumed at breakfast, may be presumed to be articles more likely than others to furnish to the blood the offending material.

I have not attempted to exhaust this interesting subject, but have for the present limited myself to pointing out the occurrence of a form of albuminuria which, so far as I am aware, has not in its details been previously described by any observer, namely, albuminuria occurring during and after digestion in cases of vascular bronchocele and exophthalmos.—*Edinburgh Medical Journal, April,* 1874, *p.* 880.

29.—ON THE TREATMENT OF DIABETES MELLITUS WITH CARBOLIC ACID.

By the EDITOR of the MEDICAL TIMES AND GAZETTE.

Dr. W. Ebstein and Dr. Julius Müller, of Breslau, have published in the Berliner Klin. Woch. of Dec. 8, 1873, an account of some observations on carbolic acid in diabetes, and though their facts are at present too few too warrant any decided opinion on the subject, yet they are worth recording, in order that others may repeat their experiments on a larger scale. These authors were led to make use of carbolic acid from *á priori* reasoning. Starting from the theory that many cases of diabetes arise from an increase in the ferment which converts amyloid substance into sugar in the liver, they conceived that substances which are known to arrest fermentation would be likely to diminish the formation of sugar. Prout, Griesinger, and Julius Vogel had all previously made trial of creasote (a body chemically allied to carbolic acid) in diabetes, and found it inert. Carbolic acid has several advantages over creasote as a drug. Its composition is definite, its odour pleasanter, and it can be taken in much larger doses. The carbolic acid was given dissolved in peppermint water, strength 1 gramme (= grains 15) to 300 grammes. This quantity was taken in three days—that is, about six to seven tablespoonfuls per diem.

The first patient was a working dentist, aged 46, who had shown signs of diabetes since October, 1872, after some family trouble. There was no hereditary taint, and he had had no previous illness. He had served seven years as a soldier. He was strongly built, but had in six weeks lost thirty-four pounds weight. Sexual power was much diminished. He came under treatment in February, 1873. His urine was on the average eight litres in the twenty-four hours; specific gravity, 1032; amount of sugar, 2·86 per cent. On March 4, after taking altogether

two grammes of the acid, sugar had disappeared from the urine, and its specific gravity was 1013. His weight gradually increased from 173 to 185 lbs., and he ceased to attend at the end of July, all sugar being still absent, though there had been traces occasionally present in the interval. He returned on October 29, complaining of his old symptoms, and was found to have about 2·25 per cent. sugar again present, but it disappeared as before on recommencing the carbolic acid. The patient had a mixed diet, and never a purely animal one, during the whole course of his treatment.

A second patient, a man aged 54, had had a mild form of diabetes, eleven years, the sugar varying from 2·5 to 5 per cent. in the last few years. The disease was not hereditary. One sister died of some mental affection. He had been eight times at Carlsbad, with marked temporary diminution of the percentage of sugar, but soon after leaving it, it rose again to its former height. In May, 1873, he went as usual to Carlsbad, his urine then containing 5·75 per cent. sugar, and he left it in six weeks with only 1·43 per cent. He was immediately placed on the carbolic acid treatment, and the dose gradually raised to half a gramme per diem, and on September 2 the sugar had entirely disappeared. A previous trial of the acid before the visit to Carlsbad had entirely failed. This patient habitually adopted an animal diet. The authors give details of a third case in a man, aged 23, in whom the acid had no perceptible effect. The only case, therefore, on which positive stress can be laid is the first, and there the effects appear undoubtedly referable to the acid. The analogy of other drugs—for example, of bromide of potassium in epilepsy, the efficacy of which varies so much in different classes of cases—would lead us not to expect carbolic acid to act equally in all diabetic patients; but Drs. Ebstein and Müller certainly deserve the credit of showing that it is worthy of a more extended trial.—*Medical Times and Gazette, Jan.* 10, 1874, *p.* 38.

SURGERY.

FRACTURES, DISLOCATIONS, AMPUTATIONS, & DISEASES OF BONES, JOINTS, ETC.

30.—ON NERVOUS MIMICRY OF DISEASES OF JOINTS.

By Sir JAMES PAGET, Bart., F.R.S., Consulting Surgeon to St. Bartholomew's Hospital.

As to swelling of the whole or part of a joint, its absence may be nearly enough to prove that a joint in which there are intense pain or other signs of acute disease, or which has been long painful, or in any way seemed long diseased, is only nervous. Inflammation of a joint, either very acute or of long standing, can hardly be found without visible or tangible exudation in the joint-cavity or in the textures bounding it. But there are many cases in which you cannot apply this rule for diagnosis. A joint shrunken after long disease may relapse into inflammation without renewed swelling, till perhaps a residual abscess appears. A hip-joint may be acutely inflamed without any discernible swelling; so, less often, may be a shoulder, the exudation being too little to be felt. And, making more difficulty, swelling is sometimes evident in a merely nervous joint; not indeed considerable swelling, but enough to make a mimicry of real disease much more close. You may often see this in the loose tissue by the sides of the ligamentum patellæ. The swelling sometimes appears due to such slight exudation as may ensue in any neuralgic part; like the puffiness that may come on in facial neuralgia, or the swelling and congestion of the conjunctiva in some cases of orbital neuralgia. Such swelling is commonly transient and capricious, and the fallacy may be detected by observing that, at its greatest degree, it is not, even after long time or many repetitions, nearly proportionate to the pain or duration of the disease. For a joint which is intensely painful, with acute inflammation, or with ulceration of cartilages, should be, if not at first, yet soon after, considerably and always swollen.

In other cases, swelling of a nervous joint may be due to accidental conditions. For instance, repeated blisterings or repeated paintings with iodine will give, for a time, such thickening and puffing of the subcutaneous tissue about a joint

as may be very deceptive when added to the other signs imitating inflammation of the joint. So, again, I have seen such a condition in a very marked degree produced by the long-continued use of ice about a painful joint.

On the whole, then, the absence of swelling makes it very unlikely that a joint is really diseased; so does the presence of only a trivial swelling when the nervous and muscular signs of disease are acute or of long standing; and when swelling exists it must not be counted as adding to the probability of real disease, unless it persists and is independent of such accidents as I have already named. And remember that a sensation of swelling is not unfrequently complained of when no swelling whatever exists. It is just one of the erroneous sensations to which nervous persons are prone, as they are to sensations of unnatural fulness and of weight for which there is no substantial reason. Such a mere complaint of swelling will not deceive you if you compare the suspected joint with its fellow.

But, after all, the sign most to be relied on for diagnosis between real and nervous disease of joints is the temperature. It is so important to estimate it accurately that I cannot too strongly urge you to be always studying it. You should feel with a broad surface of your hand every joint very watchfully, comparing each that is supposed to be diseased with its fellow supposed or known to be healthy, till you learn, as you certainly may, to detect even a small difference of temperature in even a small part of a joint.

When you have learnt to feel accurately the heat of a joint, you may safely rely on it for some certainties in diagnosis. A joint that feels all over cold, or cool, or not above its natural temperature, is not an inflamed joint: whatever may be the other signs of inflammation in it, it is not inflamed; you may rely on this. In the hip and shoulder, however, this test of temperature is not easily applicable. The thickness of various textures covering those joints is too great for additional temperature to be transmitted through them. But in the knee and elbow, and other smaller joints, even those of the fingers and of the tarsus and carpus, the test is easily applied and sure; and remember always, in using it, that in most persons, the front of the knee-joint, and in a less degree, the back of the elbow, are habitually cool to the touch—cooler than the arm and leg; and that in many the feet are rarely warm in health. These local variations, however, can lead to no confusion if you follow the rule—which, for other reasons as well as this, ought to be never neglected—of comparing every suspected joint with its fellow. Besides, always compare the temperature of the joint with that of the rest of the limb, for the rest of a limb may be, through disease or long defective nutrition, cold; and if one joint in it be

always not cold, though it may not be fairly called hot, this may be through its being inflamed. I repeat, then, a joint which is not above its natural temperature is not an inflamed joint. But observe, besides, some rules within this rule.

A joint when first exposed for examination may feel over-warm. This may be due to its having been very warmly wrapped, or thickly covered with water-dressing, or in some other way hindered from cooling to its natural temperature. Wait, therefore, and observe whether, after a few minutes' exposure to cool air, the joint has become of the same temperature as its fellow or as the rest of the limb. If it have, you may be very nearly sure it is not inflamed; or your suspicions that it is inflamed may increase with the time during which its cooling is delayed. Frequently only a part of a joint is inflamed, and in this case the morbidly high temperature can be felt only over the inflamed part. This limitation of a sign of inflammation which one might suppose easily diffusible is a striking fact. It is the same with the swelling, which, especially in scrofulous inflammations of joints, is often limited to a single portion.

Not rarely when, you feel a joint quite cool, the patient will tell you that it is, nevertheless, at times very hot or burning hot, and that with this heat it swells and becomes red. Such cases are common among those in which the consequences of sprains and other injuries are very prolonged in nervous persons, or even healthy persons whose joints have been too long treated with cold douches or too long kept at rest. These joints are commonly reported as getting very hot every evening, and as being subject to painful heats, tingling and burning. In any such case you may decide that the joint is not inflamed. If it were, it should be over-warm all day and all night. The occasional heat is only due to flushing, such as some nervous people have in their faces after their meals, or such as women commonly have at the time of ceasing to menstruate. I call it flushing, not blushing, for it is not associated with any mental state; indeed it may seem strange that among all the nervous people with joints that are the constant objects of their attention, one never sees blushing of the skin over them when they are being looked at. All such turgescence and transient heat are consistent with complete absence of organic disease. Doubtless the same thing may happen in inflamed parts; they are sometimes hotter than at other times, more swollen and more painful, having periods of exacerbation; but then they are never of natural temperature—they are always more or less too hot. It is very different with the mimicries of inflammation in joints; here the over-heat is only at times, at night or at some nearly regular hour, or after exercise or fatigue. A

joint which is cold by day and hot by night is not an inflamed joint—that is certain.

The certainty of the diagnosis based on coldness is, if possible, increased by coincident duskiness of the skin—by its having the dull purplish tint which is commonly called blue or a dull pink. Such colours may be seen at joints long inflamed, but in these cases they are associated with over-heat; when they are associated with coldness, they are characteristic of anything rather than inflammation. Besides, with inflammation the colours of over-fulness of bloodvessels are only at the inflamed parts; with congestion in cold nervous joints they are commonly much more widely diffused.

Lastly, as to fever associated with a supposed inflammation of a joint. It may help you to diagnosis in some few cases, chiefly in those in which the pain is very severe, or in which there are other signs like any of those of acute inflammation. For no very acute inflammation of a considerable joint can exist without fever; and therefore, when a patient's general temperature is normal, you may be very doubtful, to say the least, whether an intensely painful joint is inflamed. But the reverse is not true; the temperature may be frequently or habitually high, though a suspected joint be only neuralgic or in some other mimicry of acute disease. For the patient may be tuberculous, or, with some casual illness, may have a high temperature, or he may be recently convalescent from acute fever. Moreover, a very slight degree of inflammation in a joint in a very nervous person may be associated with a disproportionately high general temperature. This is in accordance with a general rule already mentioned—that in those with alert and mobile nervous systems a slight local inflammation may produce or be associated with a great increase of general temperature. In similar persons, fatigue or passing excitement will raise the temperature to 100°, or, I think, to 101°; and in their convalescence from acute illness—a condition in which nervous mimicry, as well as real joint disease, is apt to appear—they have widely variable temperature. You must therefore hold, as a general rule, that in very nervous persons, the temperature must be studied many times, and with circumspection, before it is counted as an addition to the evidences of their having real joint disease. Within the last few days I have been very nearly deceived in such a case. A gentleman, about thirty, had what he described as frightful paroxysmal pain in one knee—pain such as might have been in the most acute inflammation of the joint; and there were some heat, some swelling, and a general temperature of 101°. These things had "come of themselves" within three or four days, and the patient looked very ill. Of course he was treated cautiously; and all subsided so

quickly as to make it sure that no serious, if indeed any, organic disease had existed. It was a nervous mimicry ensuing upon excessive and unsuccessful work.—*Lancet, Nov.* 22, 1873, *p.* 728.

31.—NECROSIS OF THE OS CALCIS; REMOVAL OF SEQUESTRUM BY BLOODLESS METHOD; REMARKS.

By Sir W. Fergusson, Bart.

The bloodless method is especially valuable in operations like the subjoined, where the manipulations have to be carried on in tissues in close proximity to such important structures as the ankle-joint. The absence of hemorrhage allows of a complete inspection of the diseased part, so that the surgeon can readily ascertain what he is cutting. For the notes of the following case, the interest of which is enhanced by the addition of the remarks made by Sir Wm. Fergusson, we are indebted to Mr. William Rose, surgical registrar.

F. G., a clerk, aged twenty-seven, was admitted Nov. 6th, 1873, with disease about the right ankle-joint. In January last, from no assignable cause, a slight red swelling appeared on the outer and back part of the right heel, which caused much pain and made the patient lame. The swelling increased in size, and burst in May, and discharged a quantity of offensive matter. Since this he has been laid up, and there has been a constant discharge. The pain has been less, and only occasional. He has always enjoyed good health, and there is no history of syphilis.

On admission, there was a sinus on the outer side of the right heel, an inch and a quarter behind and below the external malleolus. The opening was surrounded by fungoid granulations, and a probe passed in through it led directly to rough bone at the posterior part of the os calcis, which was evidently diseased. The ankle-joint was quite healthy; no impairment of motion.

Nov. 8th. Chloroform was administered, and the elastic bandage and india-rubber piping were applied according to Esmarch's method for temporarily arresting the circulation. Sir William then enlarged the sinus with a probe-pointed bistoury, and, after using the cutting pliers, elevator, and sequestrum forceps, removed an irregular piece of necrosed bone about an inch in diameter, representing the whole of the posterior part of the os calcis, which was lying comparatively loose in a cavity partly formed of new bone. After removing the india-rubber tubing there was only slight hemorrhage, which soon ceased; and the wound was filled with oiled lint and covered with water dressing.

Sir William remarked that necrosis of such a bone as the os calcis was not so common as caries, although it was not unusual to find a small piece of necrosed bone lying in a carious cavity. It was rather remarkable that, although the whole of the posterior part of the bone had necrosed, there seemed no interference with the attachment of the tendo Achillis. Probably the deposit of new osseous matter thrown out had been sufficient for its support.

Nov. 14th. No constitutional disturbance; wound looking healthy. The cavity is rapidly filling up.

Dec. 6th. Wound nearly healed.

20th. Will leave hospital in a day or two.

In making some remarks on bloodless operations, Sir William began by saying that the method of temporarily suspending the circulation, which had just been adopted, was that specially devised by Professor Esmarch, but that the principle was not new; that a somewhat similar process had been carried out in India years ago in cases of removal of large scrotal tumours; and also that it was the frequent practice of surgeons in this country, when about to amputate a limb, to hold it up above the rest of the body for some time before the operation to allow a great portion of the blood to gravitate out of it. He then observed that this was quite a revolution of the practice of the older surgeons, who would have been horrified at such a proceeding as this, since they were in the habit of taking ten, twenty, or thirty ounces of blood from a patient before his undergoing any capital operation. But at the same time Sir William thought that some arguments might be reasonably brought against the uniform adoption of this method; that though there were many cases in which patients could not afford to lose a drop of blood, yet there were others—for instance, in those of amputation in the thigh—where the sudden addition of so large a quantity of blood as that contained in the limb to the rest of the circulation might be more injurious than the loss of some part of it with the limb when removed. He further observed that this bloodless method had been tried by his colleagues with no bad results at present, but that there had not been sufficient time to note whether there was any difference in the progress of wounds after its use. In operations for necrosis especially, this method greatly facilitated the manipulations of the surgeon. Previously the hemorrhage from the vascular structures cut through, so obscured the part that the sense of touch was the only guide to the operator; but now the surgeon could see the sequestrum, ascertain its extent, and easily distinguish it from its new bony casing.—*Lancet, Jan.* 3, 1874, *p.* 10.

32.—TWO CASES OF DISLOCATION OF THE HIP REDUCED BY THE AMERICAN MODE OF MANIPULATION.

By Dr. HECTOR C. CAMERON, Edinburgh.

[The author has recently met with the two following cases.]

Case 1. *Dislocation on to dorsum ilii of seventeen days' standing. Reduction at first attempt.*—William Lynn, aet. 16, miner, was driving a horse harnessed to some "hutches," in a coal pit, on the 5th September, 1867. The horse ran off, and he, holding on to the reins, was drawn along by it. Being at last brought on to his knees, the hutches came up behind and struck him on the lower part of the back. When he rose he was unable to stand, experienced pain in the left hip joint, and his "legs clung together the same as if they had been tied with a rope." About two hours after the accident he was seen by a medical man, who recognised a dislocation, and, having produced extension by pulling on the leg, while he kept one of his feet pressed against the perineum, he stated, according to the patient's account, that he believed he had succeeded in effecting reduction; explaining, that the shortening and inversion, which continued to be a source of alarm to the patient and his friends, would in all probability pass off. Thirteen or fourteen days after the accident, the patient still suffering great pain, the doctor returned with a friend. Having administered chloroform, and got the assistance of "two labourers who were working on the road," they produced violent and long-continued extension of the limb, while steady counter-extension was maintained on the axillæ. They left after many attempts, and without succeeding in their object. After a day or two of great pain, he was admitted into the Royal Infirmary. I saw him after the house-surgeon in charge had made an unsuccessful attempt at reduction under chloroform. The head of the femur was on the dorsum ilii, and all the symptoms occurring under such circumstances were clearly marked. Chloroform having been fully administered, I flexed the leg on the thigh, and the thigh on the abdomen, abducted the limb fully, and rotated outwards. During this last movement, I felt the tearing through of adhesions, and the head of the bone at once returned to the acetabulum. On extending the limb, it was seen to be in good position. The patient made a good recovery, and went to the Convalescent Home three weeks afterwards.

Case 2. *Recent dislocation of the hip into the thyroid foramen. Reduction*—Thomas Yuill, aet. 42, quarryman, was admitted on the 25th April last into a ward of the Royal Infirmary, of which I was in temporary charge.

He had been struck a short time previously by the "jib of a crane," but could not give a very accurate account of the accident. He complained of great pain in his right hip and thigh, and a very slight examination served to show that the head of the femur was in the obturator foramen. There was slight lengthening and marked abduction of the limb; while the knee was slightly flexed, and the prominence of the trochanter was conspicuous by its absence. There was a distinct fulness in the obturator region, although, as the patient was a very muscular subject, the head of the bone could not be defined by the hand. Reduction was effected under chloroform in the following manner:—I flexed the limb as thoroughly as possible, slightly abducted, and then rotated inwards firmly, at the same time adducting the limb and bringing it towards the floor. This resulted in transferring the head of the bone from the thyroid foramen—not to the acetabulum, but to the sacro-sciatic notch. Flexion, abduction and rotation outwards (which so readily transfers the head of the bone usually to the acetabulum in the case of dislocations upwards and backwards), sent it back to the thyroid foramen. A careful repetition of the first set of movements put it again in the sciatic notch; and again as before, in trying to reduce it from that position, I sent it back to the thyroid foramen. Having again removed it to the sciatic notch, I put my foot in the perineum, and made extension aid my movements, when it slipped at once into the acetabulum. The time occupied was very short, for the head of the bone was changed from the one position to the other with provoking facility. The patient made a good recovery.

The first of these cases illustrates the ease with which a dislocation of the hip may be reduced when extension, fairly and intelligently tried both with and without chloroform, has failed to attain the desired result.

The second case is interesting, chiefly as pointing out a mishap which seems apt to occur in manipulating the thyroid dislocation; namely, the removal of the head of the bone not into the acetabulum, but upwards and backwards into the great ischiatic notch or on to the dorsum ilii. In a paper by Dr. Thos. M. Markoe on "Reduction of dislocated femur by manipulation," in the New York Journal of Medicine for January, 1855, this is well shown in several of the cases related. In Case IV. the original dislocation was into the sciatic notch, and Dr. Markoe says: "The effect of the first attempt was to throw the head on to the obturator foramen, making the limb longer than the other, and producing the deformity characteristic of that dislocation. From this point, by a slight alteration of the movement, the head could

be made to slip back to its original position. Between these two points it could be made to play backwards and forwards, but could not enter its socket." Much the same thing is described in several other cases. In all the cases of thyroid dislocation, it would seem that extreme flexion was employed and rotation was found to succeed, whether made outwards or inwards. It seems as if too great flexion is the cause of the bone slipping from the one position to the other without entering the acetabulum. At all events, this is the opinion expressed by Bigelow and Hamilton, and in the case related above I flexed the limb to the fullest possible extent.

Bigelow writes in regard to Dr. Markoe's case of thyroid dislocation: "These cases are instructive as showing that the head of the bone is directed towards the socket when the Y ligament is wound upon the shaft by rotation, *whether inward or outward*, and they correspond to the results of my own experiments, made before I had read them. . . . . It may be superfluous to add, that in Markoe's case, inward rotation would probably have reduced the bone, had the thigh been less flexed." Hamilton says: "In one example mentioned by Markoe, it is pretty evident that the head of the femur was thrown into the ischiatic notch, by having flexed the thigh too much, so that 'the knee touched the thorax.' Indeed, it is questionable whether it will be best ever to bring the thigh much, if at all, above a right angle with the body, since any further flexion can only throw the head below the acetabulum, when, in fact, it is already too low." In backward dislocations, on the other hand, the flexion should be firm and complete.—*Glasgow Med. Jour.*, *Aug.* 1873, *p.* 444.

33.—OBSERVATIONS ON EXCISION OF THE KNEE-JOINT, AND ITS AFTER-TREATMENT BY MEANS OF DR. P. H. WATSON'S PLASTER-SPLINT.

By HENRY J. TYRRELL, Esq., one of the Surgeons to the Mater Misericordiæ Hospital, Dublin.

[Mr. Tyrrell strongly recommends the splint which forms the subject of this paper as the most convenient, the safest, and by far the most comfortable splint for the patient, and also as the one requiring least after-care on the part of the surgeon. Dr. Watson, in his work "on Excision of the Knee-joint," published by MacLachlan and Stewart, Edinburgh, gives the following description of his apparatus for the after-treatment.]

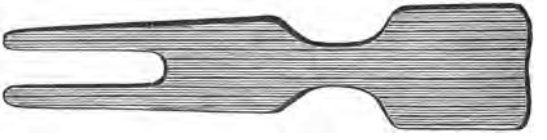
It consists essentially of two parts—1, a suspension rod made of iron, about the size of No. 5 of trade wire gauge; 2, a

modelled Gooch (scored) splint, long enough to extend from the tuberosity of the ischium to beyond the heel.

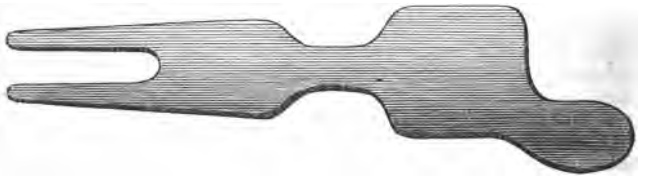
The suspension rod extends from the groin to the extremities of the toes, and is bent to the outline of the limb, departing from it only in the situation of the excision, where it forms a bow or arch. To the upper surface of the rod are attached one or more hooks, by which suspension is effected.

Suspension rod for front of limb. The arch corresponds to the site of the excision. The suspension hook to the ankle-joint. Upper end should terminate at the fold of the groin.

The Gooch splint should not be made too wide, and should certainly not surround the thigh and leg more than two-thirds of their circumference. It should be scooped away laterally, at the point corresponding to the tendo-Achillis and heel. The inferior extremity of the splint is thus of a horse-shoe or stirrup-shape, and admits of the ankle and foot being supported by the lateral horns of the splint, as they fold on each side of the malleoli, without the risk of inordinate pressure being made upon the tuberosity of the os calcis.



Posterior "Gooch" splint of ordinary form, hollowed out in each side, on the level of the operation wound, and cut out at the lower part in a horse-shoe or stirrup form, so as to relieve the heel and tendo-Achillis from pressure.



Alternative form of "Gooch" splint for excision, hollowed out as in Fig 2. The rounded part above adapted to the os innominatum, and secured by means of strips of adhesive plaster. *(This latter splint I have not used.)*

In application, the limb is first laid and carefully adjusted upon the posterior splint, which should preliminarily be padded with lint, and covered with gutta-percha tissue, or hot paraffin, in the situation which corresponds to the site of the operation. The iron rod is then placed in front, and folded lint laid between it and the limb at the groin (where the rod terminates above) at the upper part of the tibia, and at the bend of the ankle. The whole is then rendered immovable by means either of plaster-of-Paris applied by the hand, of a consistence like thick cream, or of paraffin, which, having been rendered temporarily liquid by heat, is applied by a large paint-brush. When the application has solidified, the patient may then be removed to bed, and the limb suspended from the running pulley of a Salter's swinging cradle, or from the roof bar of the common iron wire cradle, employed to support the weight of the bed clothes. I do not regard it as a matter of indifference whether plaster-of-Paris or paraffin is used. Each has its advantages. The plaster-of-Paris is firmer and not liable to be affected by heat like paraffin, but it has the disadvantage of permitting soakage of discharge, it takes longer to consolidate, and when consolidated is less easily chipped through, by means of a bandage-shears, than the paraffin apparatus. I have, of late, in the treatment of excisions and of compound fractures, employed the plaster-of-Paris as the substantial substratum, and thereafter applied the paraffin over it, as soon as consolidation and drying have become complete, so as to secure its greater toughness, and its complete impermeability to fluids of any kind—whether blood, or serum, or pus—from the line of incision, or water from the wet dressings which may be applied during the healing of the wound. The foot and limb should not be much raised above the level of the mattress upon which the patient lies, as this is apt to lead to an awkward position of the limb when the patient first begins to move about. When in bed, the sacrum and hips must be protected from all risks of inordinate pressure by means of a large square corrugated air-cushion, completely covered with a blanket or draw-sheet. This will be found to be most evenly and comfortably supported upon a firm hair mattress, laid either on a thick hair palliasse, or, better, a spring bed. The patient should be encouraged to sit up as much in bed as he can, even within a day or two of the operation; and, as soon as it is possible, should be removed out of the bed during the day, either to another bed or couch, or, should such not be attainable, to a mattress laid on the floor. In sitting up, the air pillow, placed beneath upon the seat, will be found a great comfort, as it admits of easy relief to the irksomeness of maintaining nearly the same sitting posture for a period of several hours.

Case 1.—Mary D., aged fourteen years, was admitted into the Mater Misericordiæ Hospital on the 1st of March, 1873, for disease of the right knee-joint.

For the last two years she had been in various hospitals, at different times; and, her friends not being willing to allow her leg to be amputated, she left, without deriving any benefit from treatment.

On admission the knee was found to be much enlarged, white in colour, and when at rest she did not complain of much pain; but when the heel was percussed, or the leg rotated, she complained bitterly. There were three sinuses discharging pus—one opposite the external condyle, two corresponding to the inner side of the head of the tibia. A probe passed through the openings entered the joint, and gave the sensation of grating against roughened bone. On rotation of the leg, and placing the hand over the knee, it became apparent that the cartilages were gone, and the ends of the bones bare, irregular, and rough. The leg hung like the handle of a flail, and was much atrophied. The little girl was very thin and exhausted from long confinement, but there was no evidence of any visceral disease. She stated that three years before she fell and hurt her knee, and that since she had been a cripple.

She was ordered to remain in bed, a splint was applied to the leg, and every attention paid to her general health; and as it was evident an operation would be required, I ordered her 15ℳ of the tincture of perchloride of iron three times daily (a practice I invariably adopt when time permits before performing any serious operation, as I believe, to say the least of it, it tends to lessen very much the liability to erysipelas and pyæmia).

Looking on the case as one of primary disease of the synovial membrane, believing the bones to be but little affected, and the girl's health tolerably good (also she was most unwilling to lose her leg, and begged of me not to cut it off), I determined to excise it.

Knowing how difficult it has proved, in all the cases of excision of the knee that I have seen treated in Dublin, to keep the parts in position with the usual box splint—how irksome and constrained the patient's position must of necessity be—how unfavourable such a position is to a speedy convalescence—how it must necessarily render the operation more dangerous to life—I determined, if possible, to avoid using it; and having had considerable practice in the use of plaster-of-Paris in the treatment of simple and compound fractures and diseased joints, I was struck with the simplicity of Dr. Watson's apparatus, and made up my mind to give it a trial. Before doing so, however, I tested it in the following manner:—I gave Dr. Watson's pamphlet to Mr. Corcoran, of the firm of M'Adam and Corcoran,

instrument makers, and desired him to measure my patient's leg and thigh, and to make the scored splint and suspension rod according to his directions. Having procured the splints and rod, I applied them to the diseased leg, and allowed the apparatus to remain on for two days. It proved, so far, a decided success; my patient expressed herself pleased with it; it gave no pain, allowed her, when it was suspended by a piece of bandage from an ordinary bed-cradle, to turn and shift her position with the greatest ease and comfort.

As the suspension rod moved a little from side to side, I directed the instrument-maker to make another for me, adding two small flanges of tin, each about two inches in diameter, which (I hope to be pardoned for saying) is, I think, an improvement, as it entirely prevents *wabbling*.

The flanges, being thin, are easily bent to correspond with the convexity of the leg and thigh.

On the 26th of March, 1873, I operated in the following manner:—

The patient having been put fully under the influence of ether, a tourniquet applied, and an assistant having taken charge of the sound leg, I entered the point of a strong scalpel above and behind the internal condyle of the femur, pushed it down to the bone, and then carried the incision downwards in a curved direction, severing the ligamentum patellæ, and then upwards and outwards to the upper and back part of the external condyle, where it terminated exactly opposite to where it commenced. I then dissected up the flap with the patella contained in it. I then removed the patella, and next, having divided the lateral ligaments, an assistant having forcibly bent the leg, I carefully cut across the crucial ligaments, and caused the condyles of the femur to project from the wound. I next cleared the condyloid surface of the femur for the saw, *as I well knew if I removed the whole epiphyseal line arrest of growth should follow*. I carefully avoided doing so by making a horizontal section from before backwards, commencing in front *below* the upper margin of the trochlear surface of the femur. I then removed a thin slice of bone from the tibia, and having clipped away with a strong curved scissors all the thickened synovial membrane, I twisted four vessels, sponged out the wound with a solution of chloride of zinc (10 grs. to the oz.), and finally, having inserted two *pledgets of tenax* (rolled into

plugs about four inches long, and as thick as the little finger) into the most depending angles of the wound, the limb was straitened, and I commenced to apply the apparatus in the following manner:—I first carefully wiped away all blood from the thigh and leg, and thoroughly dried the surface. I next applied a thin flannel roller from the toes to the groin (except for six inches at the knee), and then adjusted the limb on the posterior splint, which was previously well padded with fine cotton wadding, particularly in the centre corresponding to the popliteal space, and opposite the two malleoli. A piece of gutta-percha tissue was laid on over the cotton wadding in the popliteal space. The iron suspension rod was next applied in front, and a little wadding placed here and there under it made it lie accurately. A number of coarse muslin bandages, each three inches broad and six yards long, having been previously prepared by rubbing plaster-of-Paris into them, and rolling them up dry, were immersed in water, and quickly applied from the toes to the bow of the iron rod, and then under the knee and up the thigh to the groin. The bandages were applied three-fold, and a little fluid plaster afterwards, laid on with the hand, strengthened and at the same time rendered the whole apparatus more artistic looking. The edges of the wound were united everywhere but at the angles (here, as before mentioned, two plugs, or, more properly, two most efficient drainage tubes, were inserted) by four carbolized gut sutures. The splint in the neighbourhood of the knee was then well painted with fluid paraffin, and a large pledget of tenax *covered* with muslin was placed on the wound. The ends of the lateral drainage tubes were placed in the tenax over the wound, and a bandage, pretty tightly applied round the knee, under the iron bow, kept all secure. The patient was then removed to her bed, previously prepared by placing a firm hair mattress on a spring-bed, and the limb was suspended by a piece of bandage passed from the lower hook to the roof of a wire bed cradle—the heel being raised one inch from the bed. When she fully recovered from the anæsthetic effects of the ether, she became very restless—in fact, she became quite drunk and difficult to restrain. She sat up in bed, and wanted to get up altogether. Towards evening she became more tranquil, took some strong beef-tea, and a glass of wine, and at ten o'clock p.m. she had a draught containing 15 ℥ of Battley's sedative and 15 grs. of chloral hydrate. Immediately after taking the draught she fell into a tranquil sleep, and did not awake till six o'clock the following morning.

March 27th. Is a little hysterical, but otherwise perfectly comfortable; not much pain in the wound; pulse 95. Ordered to take beef-tea, milk, and arrowroot biscuits, and 4 oz. of wine; the anodyne to be repeated if required.

March 28th. Did not require the anodyne; pulse 92; slept well.

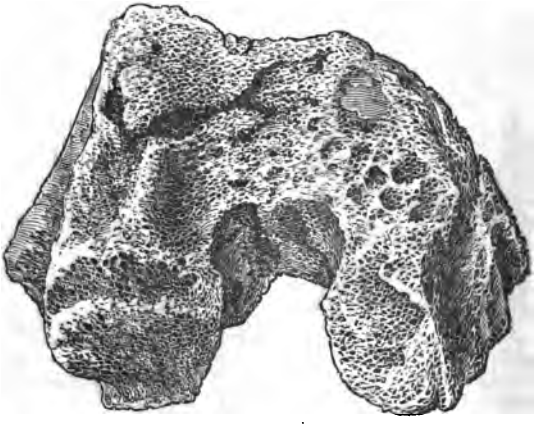
March 29th. Slept well; took her breakfast sitting up; settled her hair herself; says she is quite comfortable.

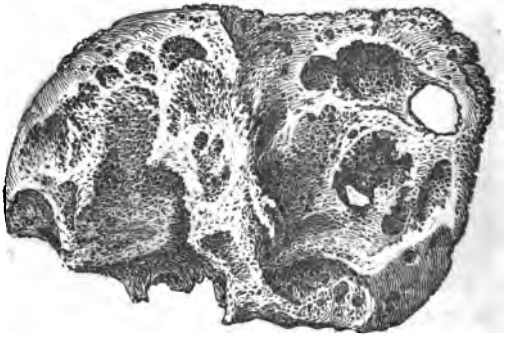
March 30th. Passed a good night; pulse 88; removed the dressing, and drew out the drainage tubes. The dressing was perfectly saturated with a sanguineo-serous discharge. Except where the drainage tubes were inserted the wound is quite healed.

(The wood engravings are taken from photographs executed on Nov. 20, and they speak for themselves.)

Before applying the tenax dressing again I softened a piece of paraffin with my fingers sufficiently to permit me to insert it like a wedge between the skin and the plaster bandage at the angles of the wound. This efficiently prevented any discharge from trickling into the popliteal space.

April 10th. Every second day the tenax dressing was renewed since last report, and before re-applying it the whole exposed surface of the knee was sprayed with a weak solution of carbolic acid (10 grs. to the oz.). There is a moderate discharge of pus from the inner angle; the outer angle almost healed. The gut





sutures have come away in the usual way by absorption of the portions inserted into the living tissues. She sits up every day, moves about from one side of the bed to the other. Several surgeons who have seen the case are quite pleased with the apparatus, and Dr. Ladly, from the United States, remarked that the only part of her person she could not injure by falling out of bed was her resected knee.

May 21st. Removed the splint to-day for the first time; union perfect; slight mobility; operation wound long since healed. The openings of two of the sinuses present before the operation not quite healed. Re-applied the apparatus more lightly; cut off the portion of the splint projecting beyond the foot; allowed to move about with crutches.

June 14th. Walks with one crutch; spends most of her time in the pleasure-grounds of the hospital.

Nov. 20th. Splints all removed; perfect bony union; she walks and runs with perfect ease. A cork heel in the inside of her boot, $\frac{1}{4}$-inch thick, prevents limping, and as she walks or runs no one would suppose she had been subject to so severe an operation.

The wood-engravings, natural size of the portions of the bones removed, show accurately the amount of disease present, and the peculiar worm-eaten appearance of the articulating surfaces of the femur and tibia. The cartilage was also removed from the patella, but otherwise it was not diseased.

Case 2.—Thomas C., aged ten years, a deaf mute from St. Vincent's Asylum for the Deaf and Dumb, was admitted into the Mater Misericordiæ Hospital on the 10th of March, 1873, under the care of Mr. Tyrrell.

On admission we were informed that the boy had been labouring under disease of the knee-joint for two years. The affection did not arise from accident. The disease was at no time attended with much pain, and until six months ago he was able to get about with a crutch. Since then he preferred remaining in bed. The medical officer of the institution had employed various remedies, but without benefit.

On examination the joint was found to be very much swollen, globular, elastic to the touch, painful when pressed on, and at three points fluctuation could be felt. The boy was small for his age; had been always delicate and dull; his head was large and misshapen. He had a slight cough, but there was no evidence of organic disease of the lungs, kidneys, or any of the other viscera. The case was evidently one of pulpy degeneration of the synovial membrane in an advanced stage, and I recommended amputation; this, however, the boy's father would not hear of.

Under these circumstances I gave a trial to rest and Scott's bandage locally, and the administration of cod-liver-oil and iron internally. This treatment was continued for a month without benefit. Three abscesses formed on the knee and burst externally, and on examination with a probe were found to communicate with the joint. Amputation was again proposed, but would not be permitted. Although I looked upon the case as one unsuitable for excision, owing to the enormous thickness and diseased state of the synovial membrane and the general unhealthy state of the boy, I gave him the chance, as I had no other alternative.

On the 25th of April I operated, as in the first case. In reflecting the soft parts to expose the bones, a quantity of thick, gelatinous fluid escaped. The synovial membrane was fully one and a half inch in thickness in some parts. It was of a yellowish brown colour, soft, and easily broken down. The cartilages were softened and easily scraped off the bones. The bones were a little expanded, but otherwise not perceptibly diseased. The patella was literally buried in the synovial membrane. I removed, with an ordinary saw, the extremities of the femur and tibia, as before described, and also dissected out the patella, and then, with a strong curved scissors, clipped away as much of the diseased soft parts as I could; much, however, was left. The wound was sponged with a solution of chloride of zinc. No vessels required to be twisted or ligatured. The immovable apparatus was applied, as in the first case, and the patient removed to bed, and the leg swung to a cradle. As soon as the little boy recovered perfectly from the ether, fifteen drops of Battley's solution were given.

April 26th. Passed a good night; seems quite cheerful, and moves about in his bed with ease.

June 14th. Everything going on well; the same dressing was used as in the other case. He hooks his leg up and lays it down on the bed himself when he is desired to do so by signs. The operation wound nearly healed; the sinuses show no disposition to heal.

July 18th. Removed the splint for the first time. No union; position perfect. Re-applied apparatus.

August 10th. For the last few days he has declined much in health. Appetite failing; he sweats at night; pulse 110; no cough: urine albuminous, specific gravity 1·010. He was removed to another ward, and shifted every day from his bed to a sofa placed in the open air.

August 17th. Complains very much of his head, the light gives him annoyance, and he excludes it by burying his forehead in his pillow, and drawing the sheet over his head. The poor little dumb fellow excites great compassion from his inability to

make known his wants. On the 20th he had a fit; his sphincters became relaxed; he lay in a semi-comatose state for three days, and then died.

The knee was the only part we were allowed to examine. The ends of the bones were in good position, but not the faintest attempt at union had taken place.

Although the result of this case was unsatisfactory, still, for my present purpose (viz., to prove how efficient and comfortable the immovable apparatus of Dr. Watson is), it answers, perhaps, even better than a successful one.

The case was an unpromising one from the beginning. The operation was not one I would have selected had I liberty to choose. Still, from the day the operation was performed till the patient died (three months), the leg gave him no trouble; when suspended from the cradle he could move freely from one side of the bed to the other, he could hook it up and lay it down himself without trouble or pain. There was no tenderness at any time since the operation over the sacrum or nates; and I believe I can, with truth, assert that with any other splint he would not have lived so long—nay, more, I do not consider the operation to have hastened his death; but, on the contrary, to have retarded it; inasmuch as he suffered less inconvenience, was less constrained in his movements, more master of his own actions, was in every way better able to assist himself after the operation than for months before. He died, not from the operation, but in spite of it.

Before I conclude this paper I shall refer to the use of tenax as a dressing, and to my mode of using it. When I introduced it first, about eighteen months ago, in the Mater Misericordiæ Hospital, it was found occasionally to give rise to extreme irritation when applied to a granulating surface, or to the skin of a delicate female (as after amputation of the breast); to obviate this I have, for the last twelve months, first placed over the wound or surface to be dressed *a piece of thin washed muslin*, and then the tenax. This mode of dressing answers admirably, and is in constant use in the hospital. It prevents the fibres of the tenax from irritating; and when you wish to change the dressing you do so with great ease and cleanliness by raising the muslin, and with it the superimposed tenax saturated with the discharge. Ordinary lint is the very worst application that can be applied to a sore or suppurating wound, its want of capillarity prevents any discharge that may be present from flowing away. After amputations, resections, and, indeed, all operations, I invariably use the tenax in the manner described, and, as a rule, do not remove the first dressings for four days.—*Dublin Journal of Medical Science, Feb.* 1874, *p.* 97.

34.—CASE OF PRIMARY EXCISION OF THE ANKLE-JOINT; WITH OBSERVATIONS.

By HENRY LEE, Esq., Surgeon to St. George's Hospital.

The author describes what he believes to be the only case in which complete primary section of the ankle-joint has been performed; and advocates the plan, both in primary and secondary excisions of the joint, of dislocating the tibia and fibula *outward* so as to allow of the articulating surface of the tibia being removed with comparatively little disturbance to the surrounding parts. The articulating surface of the artragalus is also more easily removed in this way than by dislocating the bones of the leg inward, as has commonly been attempted in secondary excisions of the joint. In many of the so-called excisions of the ankle, the extremities of the tibia and fibula have alone been removed, and the articular surface of the astragalus has been left either partially or altogether. In other cases, where a complete secondary resection of the joint has been performed, the bones have been divided by a thin saw whilst they have remained *in situ;* a proceeding, according to the author, involving considerable disturbance and risk to the surrounding parts. In other cases, again, an incision has been made on the outside of the joint, and a dislocation of the bones attempted; but this cannot be satisfactorily accomplished so long as the internal malleolus is left. The plan advocated, therefore, is to remove the internal malleolus first, and then the tibia and fibula may be dislocated outward through the external wound with great facility, and without interfering with any important structures. Such a mode of operating has not, he believes, been hitherto described. The patient was exhibited at the meeting.—*Lancet, Feb.* 7, 1874, *p.* 198.

35.—IRON-WIRE FOLDING FRACTURE BOX.

By Dr. G. W. MCNALTY, Surgeon, Army Medical Department.

Having experienced great difficulty in the Franco-German war in dressing gunshot wounds of the leg without disturbing the wounded extremity, particularly in cases of compound fracture accompanied with considerable discharge, I have been led to devise an apparatus which I trust will prove of service.

The apparatus, which may be termed an iron-wire folding fracture box, is made of galvanised iron to prevent rusting, and consists essentially of a back piece, sides, and foot-rest. The back piece is 22 in. long, $5\frac{3}{4}$ in. broad at the upper, and $4\frac{1}{4}$ in. broad at the lower end, and has an oval-shaped opening

to accommodate the tendo Achillis and heel. The sides are not so broad but are of the same length as the back piece; they are joined to the latter by two hinges on each side, and are kept applied to the limb by webbing buckled on the thick wire at the upper edges. The foot rest is 10 in. long and 3½ in. broad, and can be firmly secured at different angles by iron loops which fix on wires projecting from two small wings, thus preventing any movement either backwards or forwards. At the end of the back piece is a wire support, to raise the foot so as to prevent pressure on the heel. When not required it can be turned up, and fits into the space between the two wings.

The advantages are these:—Bandages, with the exception of the one for the foot, are rendered unnecessary. The wound can be dressed by letting down the sides unless it be confined to the back of the limb, and in most cases can be seen without disturbing the apparatus. The foot-rest remains fixed when the sides are let down; this is a most important point, as it is well known that one of the greatest difficulties in the treatment of fractured leg is to prevent either inversion or eversion of the foot. The sides and foot-rest are sufficiently high to keep off the pressure of the bed clothes. The apparatus is convenient when it is necessary to move the patient; it is also simple in construction, durable, moderate in price, light, cool, easily kept clean, equally applicable to the right and left extremity, and allows the leg to be placed on the side. All the component parts being fastened together, it will always be found complete.

The object I have specially kept in view is to make it suitable for military practice, or on occasions in civil life where a large number of persons may be injured at the same time, as in colliery accidents, &c. It is therefore constructed to pack into the smallest possible compass, the sides folding behind the back piece, the wings on one another, and the foot-rest lying flat when unfastened. The weight without pads is 2 lbs. 8 oz. Messrs. Weiss and Sons are the makers.—*Lancet, Nov.* 8, 1873, *p.* 660.

36.—A SELF-ADJUSTING SLING ARM-SPLINT FOR PATIENTS CONFINED TO BED.

By H. HAMMOND SMITH, Esq., House Surgeon to the Middlesex Hospital.

Recognising the value of Mr. Callender's sling-splint for the arm, the authorities of this hospital at once determined to adopt it; but it was found that the apparatus of Mr.

Callender was not suited for the beds at Middlesex Hospital. To remove this objection, the senior house-surgeon, Mr. H. Hammond Smith, has introduced several modifications, by which the splint may be used with almost any bed.

The apparatus consists of a wrought-iron support, a sling, and a counter-weight. The support is made of an iron bar bent at a right angle, the vertical part of which is round and works in a socket fixed by hooks or screws to the bed-head, so that it can be moved to right or left readily by the patient when necessary. At the angle of the support is a small iron pulley over which the cord of the sling runs, and on the horizontal arm is another pulley, which can be moved backwards or forwards at pleasure.

The sling is made of strong canvas, with four holes on each side, and supported by four cords with hooks, which can be so placed in the holes of the sling as to give the arm or hand any angle or position. These sling-cords are attached to the single cord which passes over the pulleys, and has at its other extremity a bag containing shot, so that the weight may be increased or diminished as occasion requires.

The advantages of this sling are, that the patient need not constantly have his arm in one position, that the position may be altered without assistance, and that it can be applied to almost any bed. It has been used with great success in cases of cellulitis of the arm and hand, in amputation about the hand, and in one case of compound fracture of the humerus.—*Lancet, Feb.* 14, 1874, *p.* 232.

37.—A NEW APPARATUS FOR THE TREATMENT OF FRACTURES OF THE PATELLA.

By Dr. W. J. WHEELER, Surgeon to the City of Dublin Hospital.

The treatment of fractures of the patella being one of the most interesting and practical subjects connected with surgery, and being one which has occupied the attention of the profession of late years, and especially in Dublin, I thought it might not be uninteresting to bring under your notice this evening an apparatus which I believe to be in every way suitable for the treatment of such fractures, and more particularly have I been induced to do so from the knowledge that almost every method which has yet been introduced in the hope of obtaining perfect or bony union has met with objections on some justifiable ground, and has not been followed by the satisfactory results anticipated by sanguine advocates. The cases of this accident which have come under my care and observation have led me to an anxious consideration of the various apparatus

suggested, and to the conviction that their defects are obviated in the splint now before you, although I have not yet had an opportunity of testing it practically in its present form. It would be unnecessary for me, before such a society as this, to enter into a discussion on the causes and varieties of this accident; suffice it to say that it may occur directly or indirectly, either from violence directly applied or from muscular action. The fracture may be transverse (which is most commonly met with) or stellate, or vertical, multiple, simple, or compound. The transverse most usually occurs by muscular violence, a sudden action of the quadriceps extensor femoris under effort to prevent the body falling backwards (being the common cause), the knee at the time being slightly bent; occasionally however, transverse fractures are the result of direct violence. The diagnosis of this fracture is not difficult —the history of the case, the loss of power, the bulging of the synovial sac; the distinct separation of the bones, which in the transverse fracture is sometimes very great, depending partially on the amount of muscular action at the time of the accident, and afterwards by the effusion which takes place into the knee-joint. As to the character of the effusion, which is said by some to be serous, by others hemorrhagic, for my own part I am inclined to think it the former.

Before describing my splint, I must mention some of the appliances that have from time to time been employed in the treatment of this accident, in order that the merits I claim for the apparatus before you may be more clearly understood.

The first method I would mention is the plan adopted by Sir A. Cooper, namely, a leathern strap or collar, buckled round the thigh above the broken bone, another strap passing under the middle of the foot, the leg being extended and the foot considerably raised, the strap is brought up to each side of the patella and buckled to the band or collar applied to the lower part of the thigh.

The next I would mention is Malgaigne's hooks, which are composed of four claws drawn together by a screw, which act on the upper and lower fragments, and Malgaigne states that the advantage of them is that the edges of the fragments are brought into direct apposition, and that they are not tilted forwards.

The next is Mr. Wood's splint, extending from the tuber ischii to below the foot, having two hooks attached; it is placed on the posterior part of the limb.

Another plan is that recommended by Mr. Butcher; he uses a forked splint on the anterior part of the thigh, firmly bandaged to the limb, the forked part being towards the upper fragment and pressing it towards the lower.

There are several other appliances, such as india-rubber bands above and below the patella, straps of plaister, pads above and below the fragments, and others, too numerous to mention.

In the apparatus now on the table, is a hollow wooden splint, $4\frac{3}{4}$ inches wide, and extending from above the middle of the thigh to the sole of the foot, at which point a footboard is attached by means of a hinge; this splint having two transverse bars, is fitted into a long box-splint, the sides of which are forty inches long and six inches in depth, in which it travels horizontally. That portion of the splint on which the limb rests can be elevated or depressed as required, by means of perpendicular slots cut through the sides of the box-splint. Thus, the splint can be adjusted to suit a long or short leg, the limb can be elevated or lowered at pleasure, and the foot placed at any angle. This hollow splint is fixed in position by means of thumb-screws which fit into the transverse bars before mentioned; two semilunar pieces of metal, softly padded, are fixed one above the other below the fractured patella by means of leather straps which pass round the limb, the leg is secured to the splint by means of two broad web straps, one round the calf the other at the ankle; the foot can be bandaged to the foot board; a roller with each adjustment is fitted in the box-splint below the footboard. From this roller start four cords, which passing through brass sheaves, are attached, two to the upper and two to the lower metal pads by means of chains and light hooks. The roller is turned by means of a key, and acting on the cords, causes the metal pads simultaneously to approach each other, thereby bringing the fragments into apposition; the rack is covered by a brass box, which can be locked, so that the adjustment of the splint cannot be interfered with by the patient.

The advantages of this apparatus will be more briefly detailed by comparison. To those who advocate Malgaigne's hooks, I claim all the advantages without the objections; my splint provides for the position of the limb, Malgaigne makes no such provision; this apparatus will procure perfect coaptation of the fragments without penetrating the soft parts, without the pain and irritation frequently caused by the hooks. In several cases treated in London by Malgaigne's method, erysipelatous inflammation has endangered both the limb and life of the patient. It is superior to Sir A. Cooper's method, by drawing on both fragments, and can make well-maintained traction of the lower as well as on the upper fragment if necessary—and over both the plans just mentioned, by its being able to exert greater or lesser force on each fragment, as the case may require. It would be tiresome were I to enter into a comparison with all the other contrivances that have been used for this fracture.

In conclusion, I will only add, that not only is this splint suitable for the fracture for which I have introduced it, but I believe also for fractures of the thigh or fractures of the leg.—*Medical Press and Circular, Dec.* 31, 1873, *p.* 586.

38.—FRACTURE OF THE CLAVICLE TREATED BY PLACING THE ARM BEHIND THE BACK.

M. BROCA recently treated a case of fracture of the left clavicle, in which the fracture was near the middle of the bone; was oblique, from above downwards and from without inwards, the fragments riding one another considerably. After numerous plans of treatment had failed to reduce the fragments, M. Broca adopted a suggestion made last year by Dr. Michel to the Sociéte de Chir., and which is reported in the Jour. de Méd. et Chir. Prat. He placed the arm in a semiflexed position behind the back, where it was retained by a bandage for eighteen days, with the effect of completely adjusting the fractured surfaces and producing an excellent cure. For a few days longer a sling was used. The patient, a man of considerable nerve, complained of the inconvenience and pain of the method for only the first twenty-four hours.—*Medical Press and Circular, Dec.* 24, 1873, *p.* 574.

39.—ON EXSANGUINATION OF LIMBS BEFORE OPERATIONS, AND ON TORSION OF ARTERIES.

By the EDITOR OF THE LANCET.

It may be fairly asked whether something like the attainable limits of even conservative surgery have not now been reached. Within the last few years mild and compromising measures have been successfully substituted for bold and radical operations: limbs formerly condemned have been saved by the comparatively small sacrifice of a joint, and cases which in the opinion of the older surgeons would have been considered hopeless are now treated with the confident assumption of a prospect of recovery. Quite recently surgical conservatism has gained still greater conquests; for by the adoption of a method of operating for which we are chiefly indebted to the active intelligence of Professor Esmarch, a joint may now be excised, or even a limb removed, with little more than the mere sight of blood. During the present year, also, another method of performing bloodless operations has been revived by Professor Dittel, who has removed large tumours, and even amputated the thigh, by means of gradual strangulation by an elastic ligature. What will be the verdict finally passed on these pro-

cedures, time alone will show; but, so far, it appears that the exsanguination of a limb before operation is so simple, and in many cases so advantageous, that it must become a constant and universal practice. It is doubtful, however, whether the second plan of operating will find favour with the majority of practical surgeons. It is evident that, before it receives full recognition, its pretensions must be more clearly defined, and its powers more accurately determined. It is difficult to imagine that the excision of a breast or the amputation of a limb by the tedious process of strangulation will be generally preferred to the more certain and more speedy use of the knife. But for the removal of small growths, or for the removal of tumours in situations too dangerous for the employment of a cutting instrument, a strangulating ligature may prove to be of the greatest service, and free from danger.

Intimately associated with the subject of bloodless operations is that of the arrest of primary arterial hemorrhage. Practically, four methods are in use—namely, ligature by whipcord or silk, ligature by catgut or other animal substance, torsion, and acupressure. Much has been said by advocates and opponents for and against each of these methods. Some, firmly wedded to this plan or to that, will never allow any merit to a rival. The question at issue, however, is not solely the arrest of the hemorrhage, but includes also the subject of the primary union of wounds. It has been asserted that the ordinary ligature, which is acknowledged on all hands to be a most efficient mode of securing a bleeding vessel, is attended with the serious disadvantages that many days must elapse before its separation, and that the irritating thread and the distal fragment of the ligatured vessel by their presence act as foreign bodies to prevent the speedy union of the wound, and thereby increase the dangers of secondary hemorrhage and other disastrous results of operation. On the other hand, the advocates of the use of a ligature made of some animal substance, and the advocates of torsion or of acupressure, have each alleged that these evils do not occur with the employment of their favourite method. The reintroduction of ligatures made of some animal substance said to be capable of absorption, and the use of torsion of arteries, are mainly due to the efforts of those who practise antiseptic surgery; who argue that when the vessel is secured with a substance such as catgut, the ligature becomes organised and incorporated with the surrounding tissues, so that no dead fragment is left in the wound to retard its union.

A similar advantage has been claimed for the torsion of bleeding vessels. The principles of torsion are based on a knowledge of the anatomy and physiology of arteries. The arteries contain in their walls a muscular tissue, which contracts

and retracts whenever the vessel is divided, the more so if it is rudely lacerated. This muscular tissue, which exists proportionately in the greatest degree in the small vessels and in a smaller degree in the large ones, is ruptured when an artery is twisted. But the unequal distribution of muscular tissue is a serious objection to the exclusive application of torsion. Another objection is to be found in the fact that the arteries are liable to be extensively diseased, so that the muscular tissue is changed or degenerated in such a manner that it no longer fulfils its normal functions. If, therefore, arteries so affected be twisted, there is a danger lest the process necessary for the temporary and permanent closure of the vessels shall not take place. Torsion is, moreover, not adapted for cases where the artery cannot be sufficiently withdrawn to allow of free twisting, and is useless where hemorrhage occurs from the side of one vessel, as when a branch has been divided close to the trunk. It appears, therefore, that torsion requires for its complete and safe employment a healthy subject, sound arteries, good instruments, and a persevering operator whose acuteness of vision is very great, for the difficulties of torsion are largely increased if the sight be at all defective; it will, indeed, generally be found that, as surgeons advance in years and become presbyopic, they discard torsion for the more simple procedure of the ligature. While, then, the principles of torsion are, that by twisting the vessel the muscular coat is ruptured and becomes contracted, and that its roughened edges act as foreign bodies, which whip out the fibrin, and on which lymph is subsequently deposited, it seems practically to be a matter of small importance whether the end of the vessel is twisted off, or whether only a few turns are made. But the question in dispute is whether the bruised and twisted end sloughs off or retains its vitality. The truth is, that much depends on the size of the vessel and the manner in which torsion is performed, for if the piece have not been severely bruised it may preserve its vitality; but if it have been twisted too frequently, or if the tissue be much broken, there can be no doubt that the end does often slough off and act as a foreign body.

Notwithstanding the strenuous advocacy of its ingenious originator, acupressure has not gained much favour with English surgeons, and it may indeed be fairly doubted whether it is a means deserving of implicit confidence. There is no proof that the use of the needle produces division of the muscular coats, which experiment and experience have shown to be a very important factor. It is no proof to say that it is frequently successful, for at least it is liable to failure for the reason just stated; and it is not safer than the ligature or

torsion, over which it cannot truly be said to possess any advantage sufficiently great to compensate for the risk incurred by its use.—*Lancet, Dec.* 20, 1873, *p.* 883.

40.—AMPUTATION AT THE HIP-JOINT BY ESMARCH'S METHOD.

By Dr. CHARLES J. GIBB, Consulting Surgeon to the Newcastle Infirmary.

Having occasion last week to amputate the left leg of a youth eighteen years of age at the hip-joint for malignant disease of the femur, I did so by Esmarch's method, and the result was so satisfactory that I desire to record the case, more especially as, so far as I am aware, this is the first time amputation at the hip-joint has been performed by the bloodless method. The tumour was a very large one, and the limb œdematous. The elastic bandage was applied from the toes to the perineum, a good-sized pad of linen placed over the aorta, just below the umbilicus, and seven or eight turns of elastic tubing made over it. Not a drop of arterial blood was lost till the ligature was loosened, and then only one vessel spouted, all the others having been secured. When the femoral vessels were divided, about two ounces of venous blood escaped. No inconvenience was experienced from the application of the tubing tightly round the belly, and after the operation neither the lad's countenance nor his pulse indicated that any great shock had been sustained.

I am indebted to Dr. Page, the house-surgeon to the Newcastle Infirmary, for suggesting to me to try Esmarch's plan in this case.—*Lancet, Jan.* 31, 1874, *p.* 181.

41.—EXTENSIVE DISEASE OF KNEE-JOINT; AMPUTATION THROUGH THE THIGH; BLOODLESS OPERATION; RECOVERY.

Case under the care of J. KELLET SMITH, Esq., Stanley Hospital, Liverpool.

[The patient was thirty-nine years of age, and had suffered from knee-joint disease for four years.]

Although the case seemed very unsuitable for the employment of Esmarch's bloodless method, it was determined at any rate to try it, and at first it was thought that excision of the knee might be performed.

On Nov. 7th the patient was put under chloroform in the ward to allow of his being removed to the operating room. Upon his arrival there the chloroform was discontinued, and

ether used instead. The limb was tightly bandaged, from the toes to the upper third of the thigh, with elastic bandage about four inches in width, after which a stout elastic cord was worked spirally over all until it reached the last turn of the bandage, when it was fastened, and the bandage removed. An incision was then made on the inner side to fully expose the knee-joint, which was found to be so extensively diseased that amputation was decided upon. The incision made on the inner side of the limb was continued forwards, embracing the patella, and brought to the outer side, thus making the anterior flap. The knife was then passed behind the femur, commencing at the same point as the anterior, was carried forward for a short distance, then abruptly downwards, thus making a short posterior flap, about one-fourth the length of the anterior. After the arteries, which were very much enlarged, had been tied with carbolised catgut, and the part well washed with carbolic-acid lotion, the flaps were brought together and secured. A piece of spiral wire drainage-tubing was passed through the part, and made to project about an inch on each side; some pieces of lint soaked in carbolic oil were then put on the part, and the patient removed. Upon making the first incision on the inner side of the knee a large amount of pus, and about an ounce of clot escaped, but during the rest of the operation there was no blood lost. The patient complained of no pain, such as was supposed would be caused by the sudden gorging of the vessels with blood, upon the removal of the cord.

The convalescence was rapid, there being no bad symptom throughout, and the patient has regained a florid, healthy look; he has also increased in weight.

At this operation, in spite of the excessive œdema, the elastic compression and bandaging most effectually succeeded in making a bloodless operation, and although the pressure was kept up for half an hour there was no sloughing. In a fortnight firm union had taken place in the deep parts of the stump. —*Lancet, Dec.* 20, 1873, *p.* 878.

42.—ON BLOODLESS OPERATIONS AS ILLUSTRATED BY THE USE OF THE GALVANIC CAUTERY.

By THOMAS BRYANT, Esq., Surgeon to Guy's Hospital.

[The principle of Esmarch's practice is not new, having been acted upon in Guy's Hospital for many years, more particularly by Mr. Hilton. The extension of the principle, and the exact mode of its application contain, however, much novelty, and Professor Esmarch has made it a practical success.]

There are, however, associated with the method under consideration some disadvantages of no mean importance, and the greatest of these is the prolonged and general oozing of blood from the tissues which invariably follows the removal of the tourniquet. I do not mean the arterial bleeding that takes place from the arteries, large or small, for such can be effectually controlled by well-applied torsion, but I mean the capillary oozing of blood from the general surface of the wound; the soft parts, as it were, on the removal of the constricting band, filling like a sponge and oozing blood in a passive way; the capillaries, that have been entirely deprived of their blood by the elastic pressure, apparently losing their contractile power for a time and in this way giving rise to a passive hemorrhage; this general oozing converting what might have been a bloodless operation, so far as the mere cutting was concerned, into an operation as a whole of a totally different character, and tempting me to think that it would be wise to describe the operations performed by Esmarch's plan as examples of bloodless operating, reserving the term bloodless operations to those performed without the loss of blood from the beginning to the end.

How far, again, this secondary oozing of blood may interfere with or prevent the immediate or primary union of a wound is not yet proved. That it must do so in a degree is palpable to all, for you must know that oozing to any extent between the edges of a wound or between flaps necessarily prevents rapid repair.

Care is likewise called for in the application of the elastic band, for too tight compression must do harm. Langenbeck has already recorded three instances in which, by this pressure applied to the upper extremity, some paralysis of the median nerve followed.

In aged subjects, and in others in whom the vessels are diseased, the practice does not commend itself, for in such the complete obstruction to the flow of blood into the tissues is likely to be followed by sloughing or want of action. The same result is likely to arise when the carbolic-acid spray is employed after Lister's system; the influence of the spray upon bloodless tissues having an evil tendency,—at least so it has appeared from some cases which have occurred in the practice of my colleagues.

With respect to the bloodless method of operating by the elastic band, as suggested by Professor Dittel, one word must be said, although that word at present cannot be in its favour. It had its origin in the remarkable case of a girl, aged eleven, who died in March, 1872, from meningitis, which was

found after death to have originated from the gradual pressure of a thin elastic cord, such as that employed to keep a hair-net in position; this cord, by its constant pressure, is said to have produced a circular furrow around the skull-cap, even through the bone, and in parts through the dura mater. From this case Professor Dittel was induced to apply the elastic ligature to others, and from November, 1872, to the end of 1873 he has by means of the elastic ligature cured nævi, cases of fistula in ano, and sinuous passages. He has, moreover, removed by it a cancerous breast, amputated three legs, and ligatured many arteries. He also recently reports that he has by the same means treated cases of hydrocele, phimosis, and many examples of cancerous and other tumours. In this country the practice has been taken up by Sir H. Thompson, who applied it for the removal of a breast. The success, however, of the case, as at present known, is not encouraging, the breast not coming away for twenty-eight days. The elastic ligature may be useful for the removal of pedunculated tumours —and for such cases I have employed it since 1862—and for some rare examples of anal fistula and allied cases; but for what are called capital operations I cannot at present understand its value. It possesses also one grave practical objection, and that is the fetor connected with the sloughing process; and unless its advocates can show that it possesses some equivalent advantages it will never find a place in ordinary surgical practice, although in exceptional cases it may possibly prove of use.

With these few remarks, therefore, upon what have been described as bloodless operations, the merits of which are not yet clear or proved, I propose to pass on to consider with you the clinical value of the galvanic cautery, to show how it really possesses what the other plans claim—a true bloodless character, —and to illustrate by examples the different classes of cases in which it is applicable.

It is not my intention to go far into the literature of the subject; for if I do I may get into trouble, and fail to give credit where credit is due. But I feel bound to tell you that it was Mr. Marshall, of University College Hospital, who first used a galvanic cautery for surgical purposes, although to a very limited extent; and that it is to the late Dr. Middeldorpf, of Breslau, that we are really indebted for the introduction of a splendid battery, and a set of instruments which have rendered the galvanic cautery an agent of practical value. In 1854, he introduced both, and published a monograph upon the subject. In this country, however, the practice was little known, and never followed, and it was not till 1868 that I became acquainted with it. In that year Dr. Middeldorpf sent

over to Dr. Beigel a battery with a case of instruments, and through Dr. Beigel's courtesy I had an opportunity of using them. I did so from February 26th, 1868, till the end of May, having selected from my out-patients at Guy's a large number of cases upon which to employ it. From the experience thus acquired I became so convinced of the value of the instrument that I persuaded our treasurer at Guy's to provide one for the hospital, and since that time it has been in active use. At my suggestion, Messrs. Krohne and Sesemann, who supplied the one we have at Guy's, obtained a second for themselves, which they work for private purposes; and in this way the galvanic cautery has been introduced into London practice.

There is no necessity for me to describe the battery or the instruments that are in use. You have seen them employed too often in this theatre, for a vast variety of cases, not to know all about them: not to know that the battery is a very powerful one, and that when in good working order it will heat the largest cautery we have and keep it heated—either at a white, red, or black heat; the different degrees of heat being regulated by the number of cells employed—many cells representing a high temperature, a less number a moderate one. You know, also, that the number of cells required is much determined by the thickness of the wire or cautery-iron—a thin wire or small cautery being more readily heated, and kept heated, than a thick wire or large cautery.

The battery is a Bunsen's. They are not quite like those you have seen me use in this hospital, but they are of equal if not of greater value, and the battery is more portable. They are those which Mr. Krohne has for private use, and which he has been led to make. They represent the improvements which a long practical acquaintance with the instruments has produced.

I trust I shall be able to show to you that it is with such a battery as this, and with such instruments, that the true bloodless operations are to be performed; and that, in comparison with this method, all other so-called bloodless methods are delusions. But, in saying this, I must impress upon you that to secure this result much care is called for and much discretion. The battery must be in good working order, and the instruments complete and whole: no make-shift will suffice in any way. If the battery does not work well, the required heat will not be maintained; if the instruments are imperfect, some break in the galvanic current may take place, and thus failure must ensue. As a matter of precaution, before operating, the surgeon should, therefore, test the battery and instruments that are to be employed, and in

this way save trouble and prevent disappointment or, possibly, failure.

I will now proceed to discuss the different classes of cases to which the cautery is applicable; admitting at once the charm the practice possesses, and understanding to a degree how Dr. Middeldorpf, its originator, was disposed to apply it to almost every branch of surgery. I propose to consider its use under different headings, and to demonstrate its value by brief records of cases. I shall commence with the operations upon the tongue, penis, and anus; and pass on to those for the removal of tumours, the destruction of cancers, and the cure of lupus; winding up my observations upon the subject by showing its value in the treatment of nævi.

On operations on the Tongue.—There are no operations of importance that the surgeon has to perform which have been more benefited and simplified by the introduction of the galvanic cautery than those upon the tongue; for there are none in which, without its use, hemorrhage is more troublesome or dangerous, and there are none, with its use, which more satisfactorily illustrates its bloodless character. Indeed, before the introduction of the galvanic cautery or écraseur, operations on the tongue were very rarely performed. During my student's career I can recall but very few instances in which this form of operation was undertaken; and I well remember even so bold a surgeon as the late Mr. Aston Key asserting that he never undertook such a case if he could avoid it.

At the present period, however, a different tale has to be recorded; for, at most hospitals, operations on the tongue are of common occurrence—at our own I may safely say that they are much looked after; and the reason for this great change in practice is, doubtless, to be found in the improved means we have at hand for the performance of the operation, more particularly the introduction of the écraseur wire and galvanic cautery. For by the use of such instruments, carefully employed, no fear of bleeding need disturb the mind of the operator; and what was formerly a very serious measure has become comparatively a simple one.

Operations on the tongue are mostly called for on account of cancer; and, when this affection exists, the sooner it is removed the better, for there are few, if any, local cancers that are the cause of more local distress, if left alone, than that under consideration, and there are few operations for cancer that give greater comfort than those upon the tongue.

I will now proceed to consider the operation for its removal, and will say again what I have said before, that, of all ways, the removal of the tongue by the galvanic écraseur is the one

to be adopted. I lay emphasis upon this opinion, not only because I believe it will be endorsed by every surgeon who has had any experience with the galvanic écraseur, but because at no very distant date a learned teacher of surgery in this metropolis, in a published lecture upon cancer of the tongue, went so far in an opposite direction as to say that, "to remove the disease, the least painful, most manageable, and most effective plan is no doubt by the application of the knife or scissors." In his experience this opinion was doubtless correct; but such an opinion, I venture to suggest, could only have been given in ignorance of the value of the galvanic or even of the wire écraseur.

The operation.—For the removal of a cancerous nodule of the tongue, or of a cancerous tongue wholly or in part, the first thing a surgeon has to do is to isolate the part to be removed; and this can usually be effected by the introduction of long pins, ivory pegs, or curved needles in handles beneath the base of the growth, in the way I have illustrated in my work on the Practice of Surgery, page 262; and in doing this the surgeon had better go wide of the disease. Having isolated the growth by this means, fixed the mouth open by a gag, and had the tongue drawn forward as far as possible, either by means of tongue forceps or, what is far better, a whipcord ligature passed through the tip of the organ, the loop of the galvanic écraseur is to be passed round the base of the disease behind the pins, and gradually tightened; the connexion between the poles of the battery being made as soon as the wire loop has been adjusted, but not before. The process of tightening and cauterising may then be carried out; and, when performed successfully, the part to be removed will quickly fall off, without the loss of a drop of blood. In this process of tightening and cauterising much care is called for. In the first place, the wire that is employed should be thick or twisted. I believe the twisted platinum wire is better than the thick. This wire should not be heated beyond a red heat, and the redness ought to be of a dull kind. But, above all, the process of tightening should be very slowly performed, the wire of the écraseur being screwed home only as it becomes loose by cutting through the tissues; any force may break it, and thus give rise to difficulties, or cut through the tissues too rapidly, and give rise to bleeding. Whenever bleeding follows the operation that has been described it is from one of two things—the wire cautery has been used at too great a temperature, or has been screwed up too rapidly. The surgeon had better take a few minutes longer at his operation than fail in obtaining its good effects.—*Lancet, Feb.* 28, 1874, *p.* 289.

43.—BLOODLESS OPERATIONS.

By Dr. GESUALDO CLEMENTI, Naples.

[Dr. Clementi in lately visiting the different hospitals in London, observed the frequent use of the elastic bandage to prevent hemorrhage in cases of amputation, or other operations performed on the extremities. He desires to bring to the notice of English surgeons the fact that both these methods had been practised and publicly described for several years in Italy.]

Dr. Silvestri published in 1871, in the Gazzetta Medica Italiana, (anno xiv., p. 397), that in cases of amputation he constricted the limb tightly above the part that was to be cut by means of an elastic band, the ends of which were provided with hooks so that they could be fixed without the aid of an assistant. Professor Vanzetti has latterly modified the procedure of Grandesso-Silvestri by bandaging the limb preliminarily. In a book published before Professor Esmarch read his communication to the yearly meeting of German Surgeons at Berlin, Vanzetti (Cenni storici sulla Universita di Padua, p. 163, 1873), wrote as follows:—"Following the example and instructions of the author himself (Silvestri), instead of using the tourniquet in amputations, we are accustomed to constrict the limb by means of an India-rubber band, and with so good results that ordinarily the stump does not lose a drop of blood. Before applying the elastic band, it is our custom to bandage the limb very tightly from its extremity to the point of amputation, and to keep it in an elevated position for several minutes in order that it shall retain as small a quantity as possible of venous blood."

It will be thus seen that Esmarch has only the merit of employing more generally a method already described and practised in Italy.

Billroth in the Wein. Mediz. Wochenschrift of July last, in regard to Esmarch's method remarks: "I read in a work of Vanzetti's that this procedure has been described in 1871 by Dr. Grandesso-Silvestri, and that since that time this method has been employed successfully in the Clinique at Padua."

In regard to the use of the elastic ligature in amputation of the mamma, and all the other applications of it made by Professor Dittel, in consequence, as he says, of an "accidental case" (Allgem. Wien Mediz. Zeitung, Nos. 7 and 8, 1873) I ought to state that Dr. Grandesso-Silvestri, in 1862, published in the above cited Gazzetta Medica Italiana an account of operations performed by means of the elastic ligature, and expounded scientifically the principles through which he had been led to

adopt the method. I cannot do better than quote what he then wrote:—"In reflecting on the force which this substance (gum-elastic) exercises continuously and incessantly in order to return to its natural dimensions, I thought that it might be applied to further uses, and principally to cutting the tissues without producing dangerous hemorrhages. In fact, a ring of gum elastic applied in a state of extension does of itself what an inextensible cord does when regulated by a screw-écraseur. Although slight, the pressure exerted by this substance is sufficient to obliterate the small capillary vessels, and to deprive the part with which it is in contact of the material elements of nutrition, and cause it to mortify by its incessant action. . . . . When this first thin layer has mortified, the elastic ligature exerts its power on the next, and continues thus to cut uniformly all the soft tissues. If in this march of destruction it meets a large vessel, before cutting it the internal surfaces are so applied against each other as to interrupt the current of blood, and before the coats are cut through obliteration will have certainly taken place."

After having thus so well explained the *modus operandi* of the elastic ligature on the tissues the author relates a case of nævus, of a vaginal polypus, and of a scirrhus of the mamma operated on with excellent success.

In the publication of 1871 (*loc. cit.*) he remarks that he has employed with good results the elastic ligature; (1) in occluding arteries after amputation; (2) in excision of varicose veins, angiom, and very hard fibroma of the uterus; also, from a single instance that he met with, he believes that the bones can be severed by the incessant and progressive action of the gum-elastic.

That, after his publication, the other surgeons in Italy employed his method is very clearly proved by what Vanzetti, Clinical Professor of Surgery at Padua, has written in the book I have quoted above, and which was published before Professor Dittel made his communication to the Medical Society of Vienna (Feb. 14, 1873).

Vanzetti writes: "In order to spare as much as possible bloody operations, where it is practicable, instead of the knife the elastic ligature is employed: and that since 1862, when this method, rational and not rarely very convenient, was introduced into surgical practice and published by Dr. Grandesso-Silvestri, pediculated tumours where they can be constricted at the base, hemorrhoidal tumours, prolapse and fistula in ano, nævus, and fistulous sinuses, frequently, as has been exemplified in the practice of the inventor, and in this Clinique, find in this method a sure and bloodless remedy."—*Lancet, Jan.* 3, 1874, *p.* 35.

44.—ESMARCH'S BANDAGES FOR BLOODLESS OPERATIONS.

By SAMPSON GAMGEE, Esq., Surgeon to the Queen's Hospital, Birmingham.

Impressed by the results recorded by Professor Humphry of Cambridge, I resolved (29th Nov. ult.) to test the value of Esmarch's bandages for bloodless operations. The patient, a youth aged nineteen, was suffering from acute caries of the middle third of the left femur. Chloroform having been administered and the heel well raised, I bandaged tightly, with india-rubber bandages two inches wide, from the roots of the toes, over the heel, to a point two inches above the intended seat of operation. While an assistant held his finger on the uppermost turn of bandage, the limb immediately above it was very firmly encircled with several turns of india-rubber pipe, about the thickness of the index-finger. The ends of the pipe, which was two yards long, having been firmly tied together, the elastic bandage was uncoiled, leaving the limb shrunken and waxy pale. A three-inch incision in the middle line down to the femur, free use of the gouge and chisel in the removal of a quantity of carious bone, an incision into a boggy spot inside the knee, and the passage of a drainage-tube through the opening thus made from the incision over the middle third of the bone, were the successive steps of the absolutely bloodless operation. The scanty, pale, serous exudation was barely sufficient to impart a light-yellow stain to a white handkerchief. The elastic cord once freed and allowed to uncoil, pink suffusion of the previously dead-looking limb was the work of a few seconds. Blood now trickled from the wound, but certainly not more freely than usual after such an operation. Subsequent progress has been most satisfactory. The lad is easy and feeding, the femur is reduced to nearly normal size, the discharge lessening, and the wound rapidly granulating.

In addressing the clinical class immediately after the operation, it was impossible to conceal the enthusiastic admiration of the proceeding counselled by the German professor. Only the other day one of our leading surgeons spoke as if operative surgery had reached the utmost bounds of perfection. Esmarch's perfectly simple and efficient contrivance, which any surgeon can employ with success, renders possible the utmost economy of the vital powers, and scientific precision in the performance of many surgical operations. As the lad lay on the table in deep sleep, with the long drainage-tube stretching between the pale and spotless wounds through the substance of the thigh, it was impossible to suppress grateful admiration for the triple triumph of international surgery. This country and America divide the conspicuous honour inseparable from the discovery

of anæsthesia. Chassaignac's system of drainage, to which justice has scarcely been done, is one of the greatest contributions, amongst the many, with which Frenchmen have enriched surgical art. Now, the Professor of Surgery at Kiel has introduced a method of procedure calculated to influence most beneficially, in the conjoined interest of science and humanity, a large domain of surgical practice.

The success attained in the foregoing case has led me to adopt the method in a case of excision of the knee, with the assistance of my friend and colleague, Mr. J. F. West. The proceeding was bloodless, though the hemorrhage was rather free when the ligature was removed from the middle of the thigh. The little arteries, which spirted out freely, were twisted or ligatured by our resident surgeon, Mr. Gilbert Smith, who also applied the usual apparatus to secure immobility. The case is proceeding (ten days after the operation) quite satisfactorily, with exceptional freedom from pain.

It often happens, in our profession as in other departments of human knowledge, though in varying degree, that the alluring charm of novelty seduces judgment, and soundness is sacrificed to fashion. I am always averse to magnifying the importance of new processes and remedies; but with the fullest reservation in favour of the appellate rights of experience, I look upon Professor Esmarch's process for bloodless operations as one likely to aid judicious surgeons in doing good.—*Lancet, Dec.* 20, 1873, *p.* 874.

45.—BLOODLESS SURGERY OF THE EXTREMITIES.

By J. E. KELLY, Esq., Surgeon to Jervis-street Hospital, Dublin.

Within the past fortnight I have applied Esmarch's method to four surgical cases. This coincidence induces me to detail some facts directly bearing upon the question of "Bloodless Surgery," together with a few points worthy of consideration.

Whether we concede to Esmarch the honour of an invention, or merely the adaptation of a rarely applied and undeveloped principle, is of little moment to those who derive most benefit from an expedient which we must regard as constituting one of the most striking advances in modern surgery. Simplicity and easy application are perhaps the strongest recommendations of a method, the novelty of which will be my excuse for briefly describing it. A strong elastic bandage is applied in the ordinary manner to the extremity, firmly and slowly: a piece of india-rubber tubing of the diameter of the thumb is wound very tightly round the limb immediately above the margin of the

bandage, which is then removed. The preparations are complete, and the limb is perfectly bloodless until the tubing is removed. The facility of operating under these circumstances can hardly be realised. The structures have a semi-transparency resembling that resulting from preservation in turpentine or glycerine, and as may be expected, they are picked out and distinguished with remarkable facility. The operations upon which Esmarch's method confers the greatest advantage are those in which much blood is ordinarily lost, or those performed under circumstances which prevent the operator from providing efficient aid. Amongst the former, resections and explorations, either for foreign bodies or where preliminary investigations are judicious, may be specially noticed. These preliminary measures, so conservative in their aim and results, have often been frustrated by the danger or dread of the consequent hemorrhage. Now we are enabled to pursue them bloodlessly. In provincial and in military practice a surgeon must frequently operate without skilled assistants or perhaps without any. Here again the advantages of the method are indisputable.

Against the obvious advantages of the expedient we must in justice weigh some well-founded objections. While advocating, and in some instances practising bloodless surgery, I regard it as being unnecessary in many cases, and in others as only to be used with great caution. In cases where collections of septic matter exist Esmarch warns us against the danger of favouring by pressure its entrance into the circulation. The absence of bleeding points during the operation may cause us to neglect to secure some vessels which will subsequently be troublesome. This occurred to a suggestive extent in the second of the accompanying cases, but the hemorrhage was quickly checked by the application of ice. As observed in the amputations performed in Guy's and other hospitals, the edges of the incisions may slough. I noticed this in my first case only, and my observations on it and the succeeding cases induce me to regard the sloughing as proportionate to, and dependent on the duration of the pressure by the elastic bandage, and not to the bloodless condition of the tissues. In the first of the series, being anxious to do full justice to the method, I applied the bandage in the ward before the removal of the patient to the operation theatre with the view of making the limb perfectly exsanguineous. When the patient was chloroformed the caoutchouc tubing was applied, and the bandage, after being on for about ten minutes, was removed. The results were perfect; so bloodless were the parts that in my subsequent operations I allowed the bandage to remain on but for a few minutes, and in my last, immediately after the limb was entirely enveloped, I applied the tubing and removed the bandage. This operation was completely bloodless,

and along the line of the incision not a trace of sloughing could be observed; in some parts even there was an effort at union by adhesion, but the condition of the deeper structures would not admit of such a rapid closure. I am inclined to regard sloughing as unimportant in these cases, which are really benefited by the method. In amputations and other operations, which can be completed in a few minutes, I cannot recognise the necessity of departing from the old and efficient method of controlling hemorrhage by direct arterial compression. Hence, I question the importance of a contingency which can be avoided. In military and provincial practice, where, as already remarked, the surgeon must amputate without sufficient skilled assistants, he is free to select between the immediate danger of hemorrhage and the remote disadvantage of sloughing of the flaps; observations are as yet deficient as to the constitutional effects of the expedient. The quantity of blood lost during operation is frequently a most desirable antiphlogistic. By Esmarch's method, for good or for evil, this is prevented, and severe inflammation may follow the operation; however, in such cases the surgeon can conveniently resort to phlebotomy, or other antiphlogistic treatment, as suggested by his judgment. Again, in disease or predisposition to congestion in any of the important viscera, as the lungs or brain, we may well hesitate before we throw the entire amount of blood contained in a limb, either temporarily or permanently, into the general circulation. We must also remember the possibility that this quantity of blood may embarrass the pulmonary circulation and aid in developing the fatal effects of the anæsthetic. These are points which can only be settled by experience, and they appear to me to be worthy of the consideration of the profession:—

Case 1.—*Necrosis of the Entire Tibia.*—Thos.——, æt. 17, disease of eight months' duration. Commenced with violent pain about the tuberosity of the tibia. His leg became flexed, and an abscess formed which pointed and burst about the tenth day. A series of these occurred along the front of the leg. Admitted November 10th with symptoms indicating necrosis of the entire shaft of the bone. Operation on November 15th: Commenced by an incision reaching nearly from the knee to the ankle. The new osseous case was three-quarters of an inch thick and was opened with a chisel, at the expense of much labour, to the same extent as the primary incision. The bloodlessness of the operation may be estimated by the fact that when the sequestra were removed the cavity did not contain a drop of blood. The case is progressing most favourably and the cavity is lined by healthy granulations. The edges of the incision sloughed in their entire extent, but only to a limited degree.

Case 2.—*Necrosis of Lower End of Tibia.*—Joseph ——, æt. 15, disease of two years' duration. Commenced by a pain in the ankle, and an abscess formed above the articulation. The bone became expanded at its lower third, and a succession of abscesses formed on the anterior and inner surface of the leg. Admitted October 29th, with symptoms of necrosis of the lower end of the tibia. After some general treatment the operation was performed on November 13th. A portion of expanded bone was chiselled out, and a few small sequestra were removed from the centre of the mass. This case also progresses favourably. Some smart hemorrhage occurred about two hours after operation, but it was easily suppressed by the application of ice.

Case 3.—*Necrosis of the Shaft of the Humerus.*—Margaret ——, æt. 15, disease of eight months' duration. Gives the history of rheumatic fever, for which she received hospital treatment; abscesses formed at various points along the arm; admitted November 20th, and operation on the 25th November. A long incision on the outer side of the arm, and the new osseous case, which was badly developed, was cleared of the sequestra. The special interest in this operation is owing to the application of the tubing above the arm, or round the clavicle and the neck of the scapula. Notwithstanding the protection which the vessels receive from the bones in the neighbourhood of the last stage of the subclavian artery, this operation was as bloodless as any of the preceding cases.

Case 4.—*Anchylosis of Elbow Joint; Resection.*—Anne ——, æt 23, disease of four years' duration, resulting from an injury to the joint. Admitted November 28th. Operation, sub-periosteal (November 29th), performed by means of an external incision. The osseous mass was cut through with a forceps, the end of the bone turned out and sawn off. The case progresses most favourably. The wound was dressed before the tubing was removed, and during, or after the operation, or at any time; a single drop of blood was not visible.—*Medical Press and Circular, Dec.* 17, 1873, *p.* 541.

46.—ON A CASE ILLUSTRATING ESMARCH'S METHOD OF PREVENTING LOSS OF BLOOD DURING SURGICAL OPERATIONS.

By Henry Arnott, Esq., Assistant Surgeon to St. Thomas's Hospital.

[The following is an abstract of a paper read before the Clinical Society of London.]

It happened to be probably the first operation of any magnitude in which this ingenious suggestion of Professor Esmarch

was employed in England, although Mr. MacCormac had already used it in some cases of removal of necrosed bone; and the author therefore brought forward the case as a peg on which to hang a discussion on the merits of the proceeding by the hospital surgeons present. The case was an ordinary example of old disease of the knee, in which it was determined, on exploring the joint under chloroform, to excise it at once, the patient having refused to submit to amputation. The limb was tightly bandaged with elastic webbing from the toes to the thigh, and before the removal of the bandage, a stout elastic cord was twisted firmly round the limb above the bandage and fastened with hooks. The compression was kept up for more than thirty minutes, with the effect of rendering the operation absolutely bloodless until the elastic tourniquet was removed. Convalescence was slow, but presented nothing unusual which could be attributed to the pressure employed during the operation. The points dwelt upon by the author in commenting on the case, had reference to—1. The possibility of sloughing following the use of the constricting band, and the length of time during which the pressure might be safely maintained; 2. The effect on the circulation of the compressed limb which ensues on removal of the elastic cord; 3. The difficulty of using the method without chloroform; and 4. Under what circumstances the method might be wisely avoided. In briefly remarking on these heads, it was suggested that much of the pain and sudden engorgement of the vessels with blood might be prevented by gradually relaxing the constricting band; and, in enumerating the cases in which the employment of the method might be attended with danger, reference was made to instances of septic abscess, the clot-occluded veins about a compound fracture, and limbs already gangrenous. It was also suggested that a possible risk of apoplexy was incurred by suddenly overfilling the circulatory system of aged people with brittle arteries; and that, although generally beneficial in town practice, the visceral congestions caused by the subsequent plethora in cases of large amputations in robust subjects might seriously interfere with recovery.—Mr. EASTES remarked that, in seven cases of amputations under this method, recently reported from Guy's Hospital, the edges of the flaps subsequently sloughed in five; but, in all five cases the carbolic acid spray was also used. There was, therefore, not only a deprivation of blood, but also a considerable cooling of the tissues. Doubtless, the conjoined conditions are sufficiently powerful to produce sloughing, although the elastic bandage alone does not generally appear to be chargeable with such untoward result. In cases of necrosis, &c., where a precise view of the parts included in the operation is necessary, Esmarch's method is

most useful, as the wound is absolutely bloodless.—Mr. HEATH had for a long time practised a method by which the limbs may be deprived of blood. The limb being raised, the house-surgeon draws his hand gently along the surface, so as to force back the blood to the trunk. A tourniquet is next applied, and a card placed beneath the screw of the instrument. After the operation, the tourniquet is gradually loosened, when the blood returns slowly to the limb. One great advantage of this plan is, that every surgeon already possesses all that is requisite for its accomplishment.—Mr. CLOVER said that, with an ordinary bandage and band-tourniquet, bloodless operations may be performed, as he had years ago pointed out. It had been objected to all such procedures that, practically, a little too much blood might be forced into the general circulation; respecting Esmarch's method, especially, it should be remembered that the elastic cord is apt to break readily, unless it be new. He thought that a spiral bandage is more efficacious than the hand of an assistant in forcing blood out of the limb.—Mr. MACCORMAC noticed Mr. Heath's remark, that he had not seen Esmarch's method practised. In its simplicity and readiness of application, it is superior to all other plans for restraining hemorrhage; even one drop of blood need not be lost. It facilitates and shortens operations, as sponging is not required. The bandage had been partially adopted before, but the method of Esmarch has now come into general use. Further experience must be looked for, that we may be open to all the advantages and disadvantages of the procedure. Possibly, in certain cases, a moderate loss of blood may even be beneficial. The amount of pressure required to stop the arterial current in a limb is much less than is usually supposed. The sudden entrance of blood to the limb after the operation is very agonising; a slow return, by gradual loosening of the band, is much better. In the operation for necrosis, the living bone seems dry, as well as the dead parts; why is this? The bandage cannot compress and force the blood out of the bone. It is well to apply the bandage about two minutes before the patient is quite chloroformed; and, at the time of beginning the operation, tie the cord which acts as a tourniquet around the upper part of the limb. —Mr. BARWELL considered that, of the two methods of Clover and Esmarch, that of the latter empties the limb most thoroughly of blood. In a case in which he had adopted Esmarch's plan, he had, however, found, after the operation, a good deal of oozing for twenty-four hours, principally from the bone. In another case, a case of Syme's operation, the heel was left out of the constricting bandage; upon that part being cut into, the blood at once escaped; it had not diffused itself generally through the bloodless limb. The healing process had

been rapid in Mr. Barwell's cases. He looked upon the bone as a kind of closed vessel with a small hole, from which blood cannot escape, unless fresh blood enters the limb.—Dr. KOCH (of Munich) had seen some of Esmarch's own cases, viz., an excision of the knee, an operation for necrosis of the tibia, and a little operation upon a finger. The limb was dry, and the bone was dry in each case; and Esmarch thinks that the dryness of the bone is due to the fact, that the empty veins draw or suck the blood from the bone. Esmarch has never seen sloughing of the edges of the flaps, nor any bad results in his cases.—Mr. A. WILLETT said that nothing, since the introduction of chloroform, had added so much to the comfort of the operating surgeon as Esmarch's ingenious procedure, which lends a great charm to operations. The bone is not always absolutely free from blood. After section of the bone in excision of the knee, and whilst the cord was still *in situ*, he had seen the cut surface of the bone become covered with a currant-jelly kind of clot, one-third of an inch thick. It was, of course, removed before the bones were adjusted, but even then a little more blood escaped. In only one of many cases had he seen sloughing follow the operation, which was amputation of the leg in an elderly patient for an ulcer which had extended completely round the limb. The anterior flap sloughed, and the patient sank from pyæmia. After the application of the bandage, Mr. Willett had seen the Italian tourniquet applied; but he did not think it answered so well as an elastic tube or cord in restraining hemorrhage during the operation.—Mr. H. MORRIS had frequently known Mr. Hilton (of Guy's) order an ordinary bandage to be applied from the foot upwards half an hour before an operation upon the limb; the tourniquet being also then loosely placed over the femoral artery, and tightened just at the commencement of the operation, immediately before the removal of the bandage; but, by that method, never was such immunity from bleeding secured as by Esmarch's plan. He had sometimes, however, seen such considerable hemorrhage follow operations (Esmarch's), where the cord was quickly removed, that it did away with much of the benefit claimed for the procedure. He considered a combination of plans good; such as the elastic bandage with Pettit's tourniquet, or some other plan by which the blood can be gradually re-admitted to the limb. He could corroborate the fact that, in five of seven cases of amputation at Guy's Hospital, sloughing of the flaps had occurred, and that Lister's carbolic spray, which cools the tissues, had been used in all five cases. Mr. Morris had seen death occur in an old man four weeks after a Syme's operation under Esmarch's plan. There was never any attempt of union of the flaps. In old people, the driving

of the blood from the limb is likely to prevent subsequent healing. He thought Mr. Arnott's case was one in which the procedure should not have been adopted; there was pus in the joint; the capsule might have been ruptured, and the pus forced into the blood-vessels.—Mr. BARWELL described the different methods by which the cord might be fastened.—Mr. ARNOTT did not think the carbolic acid spray had anything to do with the subsequent sloughing of the edges of the wound; he doubted whether it cooled down the limb. He thought that one very important point is the *newness* of the cord; old vulcanised material becomes very brittle. Gentlemen in the country who could not frequently renew their apparatus should remember this fact. He considered that an elastic tube tied round the limb is the best method for the compression of the arteries; small pressure under this plan will more effectually stop the current of blood than a larger pressure from the old tourniquet. —*British Med. Journal, Dec.* 6, 1873, *p.* 664.

47.—THE ELASTIC LIGATURE.

By Professor DITTEL, of Vienna.

[When the surgeon has to undertake operations in cavities and canals which are out of sight, or which are so narrow, that it is extremely difficult, if not impossible, to use cutting instruments within them, the adoption of a plan of operation which obviates all risk of hemorrhage is exceedingly desirable.]

In November, 1872, I was consulted by a rickety woman, who had her first child with her, five months old. It had, on its right temporal region, a roundish vascular growth, having a base from 2 to 2¼ centimètres (about four-fifths of an inch to an inch) in diameter. I carried a strong insect-pin through the base, and also two others, one on each side—three pins in all being thus introduced through the tumour—and over these I twisted turns of waxed thread in the way described above. As usual, I had to correct the shape of many pins which had become bent, before the affair was in the state which was desired. After some days, the circular ligature had cut into the part, and lay, with the needles, imbedded in the suppurating furrow. I should now have drawn the ligature tighter, to make the falling-off of the tumour more sure. There then occurred to me the history of a girl, aged eleven, who had fallen a victim to the refined wickedness or the extreme carelessness of an unkind stepmother. This child, Marie Kramer, was admitted into hospital on March 5, 1872. She had an extremely neglected appearance. Her hair, dirty and full of vermin, was fastened in a net; her face was pale, and her look timid. She answered

questions imperfectly and with hesitation, manifesting, evidently, that she felt herself in fear. Her statement that she had not removed her hair-net for about a fortnight, in consequence of the order of her mother, was not absolutely believed. As she complained of head-ache, the head was more carefully examined; and I found, in the part corresponding to the edge of the net, a suppurating furrow, at the bottom of which, after carefully washing away the purulent scabs, I discovered the fine elastic thread with which the hair-net had been fastened. This thread was visible in some parts; in others it was deeply imbedded and overgrown with granulations; and in some parts it lay deep in the corroded bones, especially the occipital and right parietal bones, where it had penetrated as far as the inner table. Although the furrow very soon granulated under ordinary treatment, symptoms of meningitis appeared, and she died on March 21.

The necropsy showed not only a high degree of general anæmia and meningitis, but also sloughing of the dura mater at the points where the loss of substance in the bone was greatest. The furrow in the soft parts corresponded with a furrow in the bone below the occipital tuberosity, reaching on each side over the tuberosities of the frontal bone, and thus forming a complete circular furrow in the skull. The furrow formed an almost complete chasm in the bones, so that the portion of skull lying above the furrow was connected with the bones below it only by means of remaining bridges, the whole length of which was ten or twelve centimètres—the circumference of the head at the part being forty-two centimètres.

From this act of base wickedness (for I found on inquiry, that the stepmother had not allowed the child to loosen the hair-net), I learned that an elastic cord is excellent for use in the division of tissues. And now, instead of tightening the thread in the case of my little patient above-mentioned, I applied a caoutchouc drainage-tube all round the pins. Eight days later the mother brought in the child, which had borne this ligature much more easily than the thread. The vascular growth had fallen off; and in its place was a healthy granulating surface, the circumference of which was already beginning to be covered with a cicatricial membrane. This result led me to make further researches with the drainage-tubes; and I have since then used them in the treatment of nævus, fistula ani, prolapsus ani, sinuses, cancer of the breast, and in the ligature of arteries.

The proceeding is easy, but it requires a certain care and precision, which may soon be acquired by patience and attention. The operator must avoid giving unnecessary pain through pulling the cord too tight and dragging on the part, by having

the part to be tied supported or held up by an assistant. The ligature must, of course, be drawn tight, and tied with two knots. The pain produced by the tying is altogether not great, in many cases very trifling, and scarcely ever lasts more than an hour. It is self-evident that the depth of the furrow produced depends on the degree to which the cord is tightened, and on the resistance of the tissues. It is probable that the ligature may be applied less tightly than I have done from fear of failure, as the pressure is continuous until the elastic cord has regained its former length. The division of the part is effected by the continuous pressure of the cord, which compresses the vessels and lymphatics until the vessels are plugged, and the access of nutritive material is completely interrupted. The substance of India-rubber has the peculiarity of not producing suppuration; and hence, while the compressed parts disappear, a granulating furrow is left, and after the part has fallen off, a healthy luxuriant granulating surface remains. As the tied part falls off, the ligature springs away from the granulations. The ligature then forms a closed ring, the aperture of which is so narrow that a probe or needle can scarcely enter it. The process of ligature or division lasts from three to fifteen days, according to the thickness of the pedicle and the density of the tissues; in a case of cancer of the breast, the duration of treatment was fifteen days. I have never detected febrile symptoms, even when the surface was extensive and the pedicle broad.

The mode of proceeding I have varied according to the peculiarities of the task to be performed.

1. In nævus, after transfixing it with needles as for ordinary ligature, I have the tumour fixed by an assistant during the tying. A single circular ligature is sufficient. In the two cases in which I used this treatment, the nævus fell off in eight days.

2. In fistula ani or sinuses about the rectum, the elastic ligature has especial advantages where the inner opening of the fistula lies high up or the sinus extends far. In the case of sinus, an inner opening (into the rectum) is first made by means of a trocar. The trocar being withdrawn, the elastic thread is introduced through the cannula and drawn out through the rectum. This proceeding is rendered more easy by first introducing a metallic thread into the rectum through the cannula, seizing it with the finger or forceps, drawing it through the anus, and removing the cannula. The outer end of the wire is now fastened to the small elastic tubing by means of a waxed thread. In this way the elastic cord is very easily drawn through, if the index finger of the left hand can be passed up the rectum as far as the opening. Both ends of the ligature are

drawn upon, and tied rather tight. The bridge of intestine with the sphincter is generally cut through in three or four days, and the patient finds the ligature, contracted into a ring, lying in his bed. In complete fistula ani, the metal wire is carried into the intestine along the groove of a director, and the use of the trocar is unnecessary.

3. In prolapsus ani, the protruding fold is seized with hooks or polypus-forceps, as for the application of the ordinary ligature, and drawn down a little. In order that the fold may not escape from the forceps while the ligature is being applied, an assistant must press against the fold after it is seized, a second must stretch the cord, and a third must fix between his fingers a small portion of the cord corresponding to the fold, so that it remains only for the operator to tie the ligature. The ligature falls off in three or four days.

4. In sinuses I have used the elastic ligature many times; and will briefly relate the following case. Herr Sch., residing in Odeonstrasse, aged twenty-two, very anæmic, had been suffering twelve weeks from inflammation of the left inguinal glands, following typhus. On Dec. 5, 1872, in consultation with his ordinary doctor, Dr. Hamburger, I opened an abscess; and on Dec. 7, as the glands were greatly swollen and suppuration had extended down to their lower border, I repeated the operation. There remained a sinus passing below the glands for about two inches. On December 25 I introduced through the sinus a ligature which included the superjacent skin and all the glands. On the second day, the portion of the bridge of skin that was left was only two lines long, the swelling of the glands was reduced, and healthy granulations were present. The fever disappeared; the ligature fell off on the sixth day. The patient completely recovered.

In a second operation in which I applied the elastic ligature, the bridge of skin was four inches long. The result was equally favourable.

5. In tumours, the elastic ligature has its application when they are pedunculated, or when they can be isolated, or when it is not possible or necessary to save the skin. It is thus inapplicable in diffuse infiltration. I have had only one opportunity of applying the elastic ligature to a tumour. The patient was Frau H., aged seventy-four, and at her age neither I nor another surgeon would have undertaken the risk of a bloody operation. But, having already ascertained that the elastic ligature did not produce any fever, and as the old lady earnestly desired that the tumour—a fibrous cancer of the right breast—should be removed, I concluded to make this first attempt. The cancer had at its base a vertical diameter of about four inches, and a transverse diameter of five inches, and

was movable. I passed a Fleurand's trocar through, beneath the tumour, in the direction of the vertical diameter; and, having withdrawn the trocar I introduced through the cannula two waxed threads, and by means of them drew through two pieces of small India-rubber tubing; the cannula was then withdrawn, leaving the tubes. I now, while an assistant drew forward the tumour, embraced each half in the corresponding portion of tube, and tied the ends of the tubing firmly in a double knot. This was done in the out-patient department on January 9, 1873. On the second day I had the patient taken into ward No. 81, as she was rather anxious on account of some pain. The furrow procured by the ligature became deeper daily before our eyes, leaving a granulating surface, while the tumour correspondingly collapsed, sloughed, and fell off on the tenth day. During the whole time the patient had no fever, only an increase of thirst on the eighth day. The old lady was right well pleased with the result.

6. I have applied the elastic ligature to arteries in the following cases.

a. To the popliteal artery, on the occasion of amputating the left thigh after Gritti's method. The ligature was thrown off on the sixteenth day.

b. To the anterior tibial artery in two cases of amputation of the left leg, the ligature falling off on the seventh day; and once in a Pirogoff's amputation.

c. To small branches of the anterior and posterior tibial and peroneal arteries. I have thus ascertained, that by means of the elastic ligature the flow of blood from the divided arteries may be completely arrested and their final closure perfectly attained; and further, that this ligature does not provoke suppuration. But with regard to the question whether it may be retained and encapsuled without mischief, so as to favour healing by the first intention, I have made no experiments. In the cases in which I applied it, the wounds were not closed, except in the Pirogoff's operation, where the ligature was soon thrown off.

The mode of applying the ligature to arteries requires some improvement. I have so far modified it, that the elastic thread is first applied over the end of the catch-forceps, and, when it is tied, is pushed from the instrument by an assistant, with the nail of his forefinger. The ligature then springs over the artery that is held; but sometimes it misses at first. It is probable that this ligature does not require to be tied very tight, but only just so much as is sufficient to compress the lumen of the artery, so as to allow the formation of a plug, and its organisation and definite union with the walls of the vessel, so as to completely close the artery. That the ligature remained sixteen

days in the case of Gritti's operation, must be ascribed to the fact that I connected two ligatures one with the other, so that they held on like the links of a chain. It was interesting to see the two mouths of the ends of the tube projecting from the granulations, without any trace of pus around them.

I will, in conclusion, answer by anticipation the question which will doubtless be raised. Why devise a new ligature, it will be said, when we already possess other means of strangulation, such as the écraseur, the constrictor, the platinum wire, and ligatures of various materials?

The écraseur and constrictor act indeed more quickly, but they produce much pain; they leave a contused wound, which, under all the chances which attend the process, must throw off the dead parts and granulate, to say nothing of the roughness of the proceeding, repulsive both to surgeon and to patient.

The platinum wire requires, in the first place, the possession of an expensive galvano-caustic apparatus, which is therefore beyond the reach of the ordinary practitioner; and the apparatus is so complicated, that not unfrequently, in consequence of a portion being out of order, even those who are skilled in its use are placed in difficulty. Moreover, the platinum wire does not secure against hemorrhage, and leaves a wound covered with an eschar, which is only replaced by a clean granulating wound after a rather long process of separation.

The ordinary threads have scarcely one advantage over the elastic ligature, but many disadvantages. Thread is not elastic, and thus their pressure is not constant. It acts on tissues just as deeply as it has been at first drawn together, and perhaps somewhat deeper if indirect pressure be entertained. When the first layers have been divided, the ligature becomes loose, and the tying must be repeated, at least in many cases, as in extensive sinuses and fistula ani. Thus, pain is again produced, with some bleeding from the furrow, and, perhaps, an unexpectedly rapid tying of the remainder of the tumour to avoid the danger of hemorrhage. Besides this, pus is poured out in the furrow and round the threads, and encrusts the edges, so that it becomes difficult to keep the part clean and to remove the ligature.

The elastic ligature, on the other hand, when once applied, acts gently and continuously by its elasticity, until the tissue that has been strangulated falls off, and leaves no suppuration. The shortness of the time in which the operation is completed also shows a balance in favour of the elastic ligature, and it is not much more expensive than good thread. I have no doubt that the use of the ligature will be found capable of extension to other cases, such as castration and the removal of lymphatic glands.

On account of the tendency of India-rubber to favour granulation, I have lately applied it to chronic ulcers on the feet, and to other wound-surfaces. I cover these with pieces of caoutchouc, which I press down gently by means of calico-bandages, thus at the same time excluding the air. I may say that my own observations on each case, and the patients themselves, agree that healing advances more rapidly. We generally see a small ring about one or two millimètres broad, which at its periphery exhibits daily a new transformation into cicatricial tissue. This observation is still far from being complete; it will be the subject of a future critical communication by my assistant, Dr. Mayerhofer.—*London Medical Record, Dec.* 3, 1873, *p.* 755.

48.—REMARKS ON THE METHOD OF REMOVING GROWTHS, &c., BY THE ELASTIC LIGATURE.

By Sir HENRY THOMPSON, Surgeon Extraordinary to the King of the Belgians, and Surgeon to University College Hospital.

[The following brief paper contains Sir Henry Thompson's experience upon the use of the elastic ligature in the removal of a tumour of the breast. The whole of the right breast was removed, and the patient made a satisfactory recovery.]

The progress of the case was unsatisfactory at first, owing to two circumstances. The first was an attack of erysipelas, prolonged rather than severe, which appeared two or three days after the application of the ligature, and which affected the right arm, shoulder, and most of the back.

At the end of eight or ten days the ligature was evidently not embracing the still undivided portion so firmly as it ought to have done. I therefore made it tighter, which was very easily done by pulling out the loop from the wound as far as possible without much hurting the patient, and tying round it a fine ligature, so as to diminish, perhaps by one half, the encircling noose. Certainly the mass was larger than I at first suspected it to be; and it is not to be overlooked that the whole breast was of course extirpated.

The cord on one side gave way about the second day; I therefore did not reapply it until after the other half of the breast had been divided; and on this occasion the process was more rapid, and no more erysipelas appeared as the result of the fresh pressure from the ligature on the skin. The whole mass was removed about ten or twelve days since, and nothing can be better than the appearance of the wound now, which began to diminish rapidly after the tumour came away.

I am satisfied that the fine india-rubber tube which I used, and which was the only material then attainable, was not sufficiently strong. I have therefore had some *solid* cord manufactured for me, which is much more powerful, and which I believe will divide the tissues in at least one-half the time which was occupied in this case by the tube. Its superiority is very manifest, for, being much stronger, it may be drawn at least twice as tight as the tube, and the consequence is that not only is the tension on the tumour greater, but the cord itself by the same process becomes finer, and therefore cuts more rapidly. In fact, there is no comparison between the two.

A small quantity of this cord could not be made, as no such product is required for any other purpose, and I have, therefore, deposited the whole with Messrs. Weiss and Son, from whom it can be obtained.

I also think it must be better, as Prof. Dittel now does, to apply the ligature to one-half of the breast at a time, the lower side, and not to use the second for the upper-half until the first has separated. This was what actually did occur in my case, only the upper ligature, having remained some thirty-six hours or so before giving way, strangled the whole mass, and produced a far more offensive result than would have occurred had only one been at first employed.

No doubt the principal objection to the plan is the smell which necessarily arises from the sphacelated portion. This was much controlled by constant irrigation with carbolic acid and water, from a bottle placed above the level of the patient's head; a small current flowing through a tube, the lower extremity of which rested on the breast as the patient lay in bed, the part being isolated by means of a macintosh cloth, and the solution running into a receptacle below. All this was admirably arranged and attended to by the house-surgeon, Mr. Buckston Browne.

No doubt a breast is a rather severe test for the ligature, although I do not hesitate to anticipate a very much better result with a more powerful cord. For the removal of the testicle, and for division of fistula in ano, I think it will be found admirable.

The cord itself ought to be more accurately described. Its size before use is the following: [redacted]
When applied it should be strained until it is a mere thread—say like this: [redacted]

It would be very easy to devise a simple apparatus to tighten it subsequently, but it is so easy to accomplish this by pulling it out if loose, and tying a bit of fine ligature round the portion so drawn out, that it seems unnecessary to employ any other means. But with the new cord it is very probable that no such readjustment will be required.—*Lancet, Jan.* 3, 1874, *p.*3.

49.—CASE OF FATTY TUMOUR OF THE FOREARM REMOVED BY THE BLOODLESS OPERATION.

Under the care of Mr. COWELL, at the Westminster Hospital.

The subject of this operation was a girl thirteen years of age. She presented an elastic tumour situated over the flexor muscles of the left forearm. It was oval in shape, and measured five inches in length by two inches and a half in width. An ordinary bandage, which Mr. Cowell seems to prefer to an elastic one, was applied to the arm in the ordinary way, and the solid elastic cord rendered the operation absolutely bloodless. The ordinary bandage does not empty the tissues so completely as the elastic, and the limb does not appear so blanched. The incision was made near and parallel to the median vein, which occupied a position over the long diameter of the tumour, and was more or less isolated during the rolling back of the skin. Mr. Cowell remarked that this bloodless method was a great convenience to the operator, as it made him independent of sponges during the process, but he questioned whether the wounds generally heal so quickly. He considered it necessary not to be in too great a hurry to close the wound after this method, but to wait a few minutes until the engorged condition of the surface and edges of the wound had subsided before applying the sutures, a matter which becomes of more importance after the larger operations, such as excision of the knee. In an instance of the latter operation, the application of cold water, which contained a small proportion of carbolic acid, for five minutes was most satisfactory, and the wound rapidly healed.

In this case the sutures were applied at once, and either from the separation of the surfaces of the wound by oozing, or in consequence of the partial isolation of the median vein, the wound did not heal by first intention. The wound granulated in twenty days, when the patient was discharged cured.—*Lancet*, *Dec.* 27, 1873, *p.* 899.

50.—REMOVAL OF EPITHELIOMA OF THE LOWER LIP BY THE ELASTIC LIGATURE.

By Dr. H. STANLEY GALE, Manchester.

The patient was a gentleman aged sixty-four, who said that "four years ago a small wart appeared on his lower lip, which he scratched; this caused a sore, which gradually spread until very nearly the whole of the lower lip was affected." There was little doubt as to the nature of the complaint, although a surgeon whom he had consulted lately salivated him and kept him under the influence of mercury for several weeks. He then

came to me, and the first thing he said was, "Mind, no knife-work for me." On examination I found no implication of any glands, while the patient's general health was good, and his spirits high. He consented at once to the ligature. I transfixed the lip in the centre, at a spot which was quite clear of the disease, with a narrow knife; and, passing through the hole thus made two pieces of india-rubber cord about one-sixth of an inch thick, had no difficulty in tying a knot at each extremity of the lip. The whole of the diseased surface was thus enclosed in much the same way as piles are tied after transfixion. The result was most successful. In two or three days the smell became troublesome, and I found powdered charcoal the best remedy on the whole, as it kept the parts more dry, and hence less disagreeable, than most other deodorants. On the thirteenth day one-half was detached, and on the fourteenth the second half. The wound left was a mere line, and presented a healthy-looking surface. In six days more the patient was well and able to eat his meals in the usual manner, while before the operation he could only take liquids through a tube and solids with the greatest difficulty. The patient stated that the pain was but little if any greater than that arising from the disease, which at times had been most acute.

There cannot be a doubt that this mode of operating will be of the greatest service, and must come into general use in those cases suited to its employment. During the whole time I kept him under the influence of morphia, by hypodermic injections of one-eighth of a grain night and morning. I believe this will enable many to bear the mode of operating by ligature who otherwise could not endure the pain.—*Lancet, Jan.* 17, 1874, *p.* 90.

51.—ON THE VALUE OF THE GALVANIC CAUTERY AS A CAUSTIC.

By THOMAS BRYANT, Esq., Surgeon to Guy's Hospital.

In the cases I am now about to relate, the value of the galvanic cautery as a caustic will be illustrated; and I think I shall be able to satisfy you that, as such, for the treatment of lupus, epithelial or skin cancers, and some forms of nævi, it possesses advantages such as are not equalled by any other means the surgeon has at his command. It is applied with facility, certainty, and success; and it has rendered the treatment of these hitherto troublesome affections comparatively a simple matter; for in many of the cases I am now about to relate, one application of the galvano-caustic was sufficient to set up a new action in the part, and heal a lupus, destroy a cancer, or cure a nævus.

Cases of Lupus cured by the Galvanic Cautery.—Case 1.—*Lupus of face of four years' standing; cautery; cure.*—Caroline E., aged twelve, came under my care on Jan. 4th, 1872, with a large patch of lupus covering the left cheek and extending from the left ear to the nose and chin. It had commenced four years previously as a tubercle. The tubercle at that time was cut out, but the wound never healed; indeed, it ulcerated, the ulceration spreading gradually in spite of all treatment. On Jan. 5th, with the patient under chloroform, I applied the galvanic cautery to the whole surface of the sore, and covered the parts up with a piece of gutta-percha and cotton wool. A healthy action followed this operation, and a steady cure, the patient leaving the hospital, well, on Jan. 29th.

Case 2.—*Lupus of face of four years' standing; cautery; cure.* (Reported by Mr. Manby.)—Eliza T., aged thirty, came under Mr. Bryant's care in November 1868, with a lupus covering the whole nose and upper lip. It had been gradually spreading for four years in spite of treatment. On Nov. 6th Mr. Bryant cauterised the whole surface freely with the porcelain cautery. In thirteen days the scab fell off, leaving a healthy healing surface. By Dec. 3rd, or the twenty-seventh day after the operation, the part had completely healed.

Case 3.—*Lupus of twenty years' standing cured by the application of the galvanic cautery.*—Mary W., aged thirty-eight, came to me on April 22nd, 1868, with a patch of lupus, the size of a florin, on her nose. It had existed for twenty years, and no kind of treatment had been of any service. I destroyed the whole mass at one sitting with the porcelain cautery, and charred it into a hard leathery eschar. On May 12th, when I saw her again, the surface had nearly healed.

Case 4.—*Lupus on the nose and cheek; galvanic cautery; cure.* —Mrs. H., aged sixty-five, of Fairford, Gloucester, consulted me in July, 1873, for a lupus involving the nose and cheek. It had been of some years' standing, and had spread in spite of treatment. On July 9th I thoroughly destroyed the growth by the galvanic cautery, and when the eschar came away a healthy surface appeared, which rapidly healed. Her medical attendant, Mr. Cornwall, of Fairford, reported to me in November that she was quite well.

Cases of Epithelial Cancer cured by the Cautery.—Case 1.—*Epithelial cancer of the ala of the nose; galvanic cautery; cure.*—Honore S., aged fifty, came under my care in February, 1868, with an epithelial cancer, of the size of a sixpence, on the ala of the nose. I destroyed it completely by one free application of the galvanic cautery, and a good sound cicatrix was the result.

Case 2.—*Cancer of nose of four years' standing; galvanic cautery; cure.*—Mary ——, aged sixty-four, came under my care on Jan. 8th, 1871, with a cancer involving the ala of her nose. It had commenced four years before, and, in spite of treatment, had spread. I applied the cautery to its surface and destroyed it, a rapid recovery taking place.

Case 3.—*Epithelial cancer of the nose of four years' duration; galvanic cautery; recovery.* (Reported by Mr. Ticehurst.)—Sarah B., aged fifty-three, who had had an epithelial cancer on her nose for five years and every form of treatment applied, came to Mr. Bryant in October, 1868. The growth was near the right ala, and was about the size of a sixpence. He freely destroyed it by means of the galvanic cautery; on the third day the dry eschar fell off, and the wound healed in a week. Nine months afterwards some recurrence on the spot took place, which was treated in the same way and with a like result.

Case 4.—*Epithelial cancer of the nose and eyelids; galvanic cautery; cure.*—On Feb. 26th, 1868, Mrs. P., aged fifty-five, applied to me with an epithelial cancer that involved the bridge of the nose and the inner angle of the eye. It had been growing for a year and a half. I destroyed it completely with the galvanic cautery, and a good cure ensued.

In this case on account of the position of the cancer, it would have been difficult to destroy the disease with safety in any other way.

Case 5.—*Epithelial cancer of the scrotum (sweep's); removal by cautery; recovery.*—George H., a sweep, aged fifty-eight, was admitted into Guy's Hospital, under Mr. Bryant's care, on July 8th, 1868, with a large cancer of the scrotum, the size of a crown. On Oct. 30th Mr. Bryant, with the patient under the influence of chloroform, destroyed the whole with the galvanic cautery. No bleeding attended the operation; and after the operation the man said he was in less pain than he had been before. On the seventh day the eschar came away, and the wound rapidly healed.

Case 6.—*Epithelial cancer of the scrotum (sweep's); destruction with the cautery; cure.*—James H., aged thirty-five, a sweep, came under my care in April, 1868, with an epithelial cancer, the size of a crown-piece, upon the scrotum. I destroyed it completely with the galvanic cautery, and when the eschar came away a healthy wound existed, except at one spot. This was touched again by the cautery on April 22nd, and by April 29th the parts had healed.

Case 7.—*Fungating sore over malar bone of seven years' standing; cure after the application of the galvanic cautery.*—William H., aged sixteen, came under my care in March, 1868,

with a peculiar fungating sore over the left malar bone of seven years' standing, which had followed the bite of a horse. It was about the size of a shilling, and had irregular thickened edges. I destroyed it completely with the galvanic cautery on March 12th, and by April 1st the wound had healed. On April 20th a soft pliable cicatrix existed.

On the treatment of Nævi by the Galvanic Cautery.—That nævi may be destroyed and cured by means of the galvanic cautery must be such a familiar fact to you, gentlemen, who have followed my practice at this hospital, as to make it appear quite unnecessary for me to bring the subject before you on the present occasion, and so far as some of you are concerned you are right; but as it is through you that I want others to know the value of the practice I am now advocating, I propose to point out the forms of nævi which are likely to be benefited by the treatment, and the different ways in which it is to be applied; and I shall do this as briefly as possible.

With regard to the forms of nævi that are likely to be benefited by the practice, I would remind you that nævi are met with in three varieties: (1) the purely skin nævus, that involves the skin alone, and appears as a more or less extensive and bright vascular mark; (2) the purely subcutaneous nævus, that involves the cellular tissue placed beneath the skin, and that does not affect the integument over it; and (3) the mixed form of nævus, which affects both tissues, beginning either in the skin and spreading deeply, or in the cellular tissue and growing forwards. Of these three varieties the first and the last are the more amenable to the treatment by the galvanic cautery; the first with a good certainty of securing a successful issue, and the last with a fair certainty. The purely subcutaneous may be so dealt with, but in a general way these cases are better treated by excision or by subcutaneous ligature.

In the treatment of the purely skin nævus the surgeon must take care that the whole thickness of the vascular tissue is destroyed. He must burn it down by means of the platinum point, spatula, or porcelain cautery, till the whole is destroyed and carbonised. He must see that the margins of the growth are freely cauterised as well as its centre; the base of the disease as well as its surface. He must not show any timidity in the use of the burning instrument; he must use it boldly, to the complete destruction of the vascular structure. He must so cauterise the whole nævus as to turn it into a brown, non-vascular, solid eschar. Where failure has followed the use of the cautery in these cases it has been from want of attention to these points.

In the treatment of the mixed variety of nævus the same care is called for, and the same boldness of execution; but

something additional is required, and that is the perforation of the subcutaneous portion of the nævus with the heated wire. The whole mass should be riddled in all directions: the platinum wire should be introduced into it vertically, transversely or obliquely; every part of its structure should be brought under the influence of the cautery, and under such circumstances the withering of the whole may with some confidence be looked for. In all the cases in which I have adopted this practice good success has been secured; when failure has followed it has been from a want of due attention to its details. The success, indeed, has been so good that in the purely subcutaneous variety of nævus I am disposed to think an equal amount of success is to be gained by an equal attention to the application of the cautery to the deeper parts. The success I have hitherto acquired by following this practice has encouraged me to give this opinion. The following cases will, however, best illustrate the subject.

Cases of Nævi cured by means of the Galvanic Cautery.—Case 1. Wm. G., aged ten weeks. Nævus the size of a sixpence involving the skin over the right frontal eminence. On March 5th, 1869, the growth was cauterised by means of the porcelain galvanic cautery. In ten days the slough came away, and a white cicatrix was left, no signs of the disease being visible.

Case 2.—Charles J. E., aged twelve months. Nævus involving the tip of the nose, alæ, and septum, with part of the upper lip. It had been vaccinated without any good effect. On Nov. 7th, 1865, Mr. Bryant cauterised the whole with the porcelain cautery. On the 15th the eschar fell off, and by the 30th the parts were well, no signs of nævus being visible.

Case 3.—Mary G., aged five months. Nævus the size of a shilling at the inner angle of the left eye, steadily growing. It had been vaccinated three times with no good effect. On October 30th, 1868, the whole surface was deeply cauterised and burned into a dry eschar. On Nov. 12th the scab was still adherent. 26th: The scab fell off three days since; cicatrix healthy; no sign of disease to be seen.

Case 4.—Robert C., aged sixteen months. A *mixed* nævus, the size of a sixpence, over the right forehead. On Nov. 13th, 1868, it was cauterised freely, and then punctured in all directions. On Nov. 21st the scab came off, leaving a healthy scar; and by the 26th the child was cured.

Case 5.—Agnes W., aged four months. A nævus, the size of a florin, over the right side of the forehead. It was freely cauterised on Oct. 30th, 1868. On Nov. 5th the scab was adherent, and on the 16th it came off, leaving a healthy scar. On the 26th the child was quite well. On Jan. 13th, 1869, a white movable cicatrix existed.

Case 6.—Alice C., aged four months. A *mixed* nævus, the size of a crown piece, on the side, growing rapidly. On Jan. 8th, 1869, the whole was deeply cauterised and perforated in all ways. The scab came off on the 18th, and on the 20th the parts had nearly healed. On the 28th a healthy scar existed. A month later the scar was white, and no signs of disease could be made out.

Case 7.—William S., aged five months. A nævus, the size of a shilling, on the nape of the neck, of the *mixed* variety. On Nov. 6th Mr. Bryant punctured the swelling in all directions with the heated galvanic cautery wire. The growth on the sixth day had become quite solid from inflammatory induration. In one month it had disappeared. On Feb. 1st, 1869, beyond the scars of the punctures, no signs of the disease could be made out.

Case 8.—Emily M., aged thirteen months. A *subcutaneous* nævus, the size of half a walnut, in the right cheek. It had been ligatured subcutaneously, without any good result. On Oct. 30th, 1868, Mr. Bryant punctured the swelling freely from within the mouth with the wire of the galvanic cautery. On Nov. 12th it had become perfectly consolidated, and in six weeks the whole disease had disappeared. In February, 1869, no evidence of the affection was visible.

Case 9.—Mary H., aged three months. A *mixed* nævus, two inches and a half long by an inch wide, under the chin. On Oct. 30th, 1868, the whole was freely cauterised with the porcelain galvanic cautery, and then punctured in all directions. On Nov. 12th the nævus was quite hard and solid; and on the 19th the scab came off, leaving a healthy scar, no signs of the disease being visible. On the 5th of March a suspicious spot the size of half a pea was seen and destroyed, a good recovery taking place.

Case 10.—James M., aged one year. A *mixed* nævus, the size of a shilling, in the centre of the forehead. On Jan. 8th, 1869, the growth was perforated in all directions by the heated wire of the galvanic cautery; it subsequently consolidated and then gradually disappeared, the child leaving London, cured, in six weeks.

Case 11.—Richard H., aged five months. A nævus of the *mixed* kind on the anterior border of the axilla. On the 16th of April, 1869, Mr. Bryant removed it by means of the galvanic écraseur, having passed beneath its base two hare-lip pins. The operation was bloodless. A rapid recovery followed.

Case 12.—Mary D., aged twenty-one months. A *mixed* nævus, the size of half a walnut, over the frontal eminence.

On Feb. 26th, 1868, destruction of the whole was effected with the galvanic cautery. On the tenth day the scab came off, leaving a sound cicatrix.

I have selected these cases to bring before you because they were some of the earliest I had to treat, and also because notes of them were taken by one of my then dressers. They may be read as types of the very many I have dealt with since, and of which I have not kept notes. As I described to you in my former remarks, you will have observed that in the simple nævus the mark was cauterised from its surface; in the mixed variety it was cauterised and perforated; in the subcutaneous it was perforated alone; and in one case the mixed nævus was removed as a whole with the écraseur. In all of these cases the operation was almost bloodless from first to last.

In some cases of nævi it is not necessary to destroy the whole. I have had several in which one of the eyelids was involved down to its ciliary border. In these I have been content with destroying half the growth, leaving the ciliary half alone; and I have been pleased to find that the ciliary portion of the disease subsequently disappeared, the redness gradually going, and leaving the skin white and pliable.

The same result has followed a like practice in a case in which one ear and cheek were involved, the destruction of part of the growth being followed by the withering of the whole. The most active part of the growth in all these cases was selected for destruction.

In extensive nævi, with these facts before us, it is consequently wise to deal with them piecemeal—to destroy a spot the size of a shilling at different parts of the growth; to cauterise the surface of the nævus when the skin is involved, and the deeper part by perforating it when the cellular tissue is affected; and in some cases it is well to make a clean sweep of the heated wire through the base of the growth;—the object under all these circumstances being the same—to destroy the vascular tissue, and excite sufficient inflammatory action in its deeper parts to cause what remains to wither. The larger and more extensive nævi may be thus treated with every probability of acquiring a successful result.

The cicatrices which follow the cure of these nævi by means of the cautery are singularly good. They are mostly white, and, as a rule, movable — more particularly when the nævus is thoroughly destroyed in one operation, and the eschar is allowed to dry up and drop off, the deeper parts being allowed to heal by scabbing. Why this should be I will not detain you to consider, but the clinical fact is a good one to bear in mind. —*Lancet, April* 4, 1874, *p.* 469.

52.—MATTHEWS BROTHERS' NEW "SEPTIC" CAUTERY.

The want of a small and effective lamp for heating the actual cautery is one which has long been felt. In operations such as Mr. Henry Smith's for the removal of hemorrhoids it has often been a cause of much inconvenience to heat the cautery by the ordinary bedroom fire, the bars of the grate being in many instances too high to allow the point of the instrument being inserted in the midst of a clear fire whilst the handle rests on the hearth; the alternative being to thrust the iron into the fire with the handle protruding horizontally, in which case it becomes heated in the stem as well as in the button, causing the handle to be dangerously "warm" to the operator. It may also happen that the fire in the patient's room is not a bright one, the result being that the coal adheres to the iron, from which it is with difficulty removed.

The ingenious arrangement invented for Mr. Henry Smith by Messrs. Matthews, of Portugal-street, entirely removes all these inconveniences. The Septic Lamp is on the principle of the Russian blow-pipe, by which a little methylated spirit is boiled in an upper reservoir furnished with a safety valve, and the spray of spirit vapour ignited by a little flame beneath the upper reservoir, which may be easily regulated and promptly extinguished. The jet of flame is protected by copper sides, on which the iron or irons are placed, and made hot at the button only in "one or two minutes."

The whole affair folds in a very compact little japanned box, which is furnished with a tin compartment to hold a quantity of spirit; and the whole box (which may be made to carry the cauteries also) measures on the outside 8¼ in. by 3¼ in.

We are extremely pleased with this ingenious arrangement; and have no doubt that in India and warm climates, where a fire is not readily obtainable, the instrument will be extensively used, as it will doubtless be here, owing to its simplicity, handiness, and moderate price.

The specimen lamp shown to us was fitted in a little bag, which contained, besides, Mr. Henry Smith's set of clamps, cauteries, &c., for hemorrhoids.—*Lancet, Dec.* 27, 1873, *p.* 907.

53.—ON TALIPES VALGUS.

By LAMBERT H. ORMSBY, Esq., Surgeon to the Meath Hospital, Dublin, and Demonstrator in the Royal College of Surgeons in Ireland.

The external character of this deformity may be said to be an exact contrast to varus. We find it occurring as a congenita and non-congenital affection; the congenital form, however, I

may mention, is very rare, the acquired form being by far the most frequent, coming on from a varied set of causes. It goes by the common name of flat or splay-foot, as we find a certain amount of flattening of the foot in this form of distortion. In short, valgus consists in the following: The foot is very considerably everted, the heel drawn up, the toes elevated, and the weight of the body is supported upon the inner margin of the foot, which is in a complete case abducted, flexed, and rotated outwards; we also find the longitudinal and transverse arches of the foot nearly obliterated, and in severe cases completely so, which, as a matter of course, produces that flattened appearance which is very apparent in non-congenital cases.

FIG. 1.—*Talipes Valgus.*

There are two modifications of this distortion, described in orthopædic works on the subject; as compound varieties, which I may mention are due—1st, either to contraction or shortening of the tendo achillis (which, when I say tendo achillis, I mean, the muscles forming that tendon), producing the variety called *talipes equino-valgus;* or 2nd, to elongation of the tendo achillis, producing the variety *talipes calcaneo-valgus.*

The above subdivision, in my opinion, is very unimportant, and needs only a passing notice, as the tendo achillis is more or less engaged in every case and generally requires to be divided.

Valgus is generally produced by traumatic injuries, and, as I said before, is rarely congenital.

Mr. Adams in his work on club-foot gives a very useful table which determines very fairly the degree of frequency this deformity occurs.

Table of 764 cases of congenital club-foot showing the relative frequency of congenital deformities as follows:—

Talipes varus	688
,, valgus	42
,, valgus of one foot and varus of the other	15
,, calcaneous	19
Total congenital cases .	764

In the same table mentioned by Mr. Adams we find the 42 cases of talipes valgus distributed as follows:—

Affecting the right foot only	15
,, the left foot only	10
,, both feet	17
	42

We also find two different species of deformity co-existing at the same time—viz., in one foot, say the left, we may have valgus, and in the right we may have varus, and *vice versâ*.

Causes of this Deformity.—As I mentioned before, it may be either congenital or non-congenital, and, as is natural to suppose, the causes giving rise to it in both cases will be slightly different:—

1st. Co-existence of other deformities of the hip or knee.

2nd. Deficient development of the tibia and fibula.

3rd. Rickets.

4th. May co-exist with spinal curvature.

5th. Relaxation of the plantar ligaments. Weak and delicate growing girls often suffer from a slight degree of it.

6th. Tonic contraction of the peronei muscles, together with the extensor digitorum communis; in fact, in nearly every case there is more or less shortening of these muscles and elongation or relaxation of the muscles passing to the inside of the foot.

7th. Diseases of the foot—scrofulous, affecting the ankle-joint.

8th. Traumatic causes—the contraction after the healing of a wound or a severe burn.

9th. May be produced by walking bare-footed, common among the lower classes, such as flat foot.

10th. Caused by paralysis of the muscles.

Treatment divided into (1) the mechanical, and (2) operative, or (3) a combination of both.

Treatment by Tenotomy.—The tendons requiring division of course depends on the severity of the case; in slight cases we find division of the peroneus longus and brevis sufficient; in others, in addition we have to divide the tendo achillis and extensor longus digitorum.

To Divide the Peroneus Longus.—This is easily done by placing the patient on the opposite side, and grasping the foot in one hand and making the tendon prominent. As it passes at the outside of the foot to get into the groove on the cuboid bone, insert the point of the tenotome flat, underneath the tendon, just below the bend of the ankle, then turn and cut from within outwards. When the tendon is cut through it is easily ascertained, as nothing arrests or resists the onward progress of the knife. A small pledget of lint on the puncture, a metallic splint bent to fit the deformity, a bandage applied, and the operation is completed.

To Divide the Tendons of the Extensor Digitorum Communis

and Peroneus Tertius Muscles.—Place the patient on his back or opposite side, grasp the foot in such a manner as to render the tendons prominent, insert the tenotome about the centre of the ankle in front of the anterior annular ligament to the inner side of the extensor tendon, and having got the point of the knife well behind, cut from below upwards towards the skin until you feel the tendon completely divided. If you follow out these directions there is no fear of the anterior tibial, or more properly, the dorsalis pedis artery being wounded. Then let the same pledget of lint and metallic splint be applied and kept in the same deformed position for seven or eight days, in order to allow a lymph-like organisable substance to be poured out between the divided ends of the tendons.

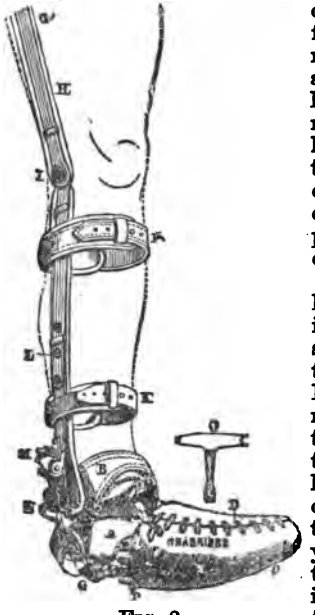


FIG. 2.

After these operative measures have been practised, I am then in the habit of finishing up with some mechanical apparatus for the permanent cure of the case. I may mention that the instrument is generally more suitable; the simpler it is in its construction, as in many cases patients like to return home at the end of the operative treatment with the intention of coming to see you occasionally to see how the case is going on, and if the instrument is very complex, the object of the appliance will in many cases, be misunderstood, and perhaps put on wrong, or applied incorrectly, and the very object you are trying to see carried out will be entirely defeated, for which reason, I say, let the instrument be simple, and apply it first yourself, and show the friends of the patient the use and utility of the appliance suggested, and then no blame, if the case ever relapse, can possibly be attached to you or your instrument.

"Scarpa's shoe" often is used for the after-treatment. Although this instrument was suggested for varus, it can with a slight modification be very easily and advantageously applied to valgus. A Scarpa's shoe made for a right foot suffering from varus will do a valgus very well of the left foot, and *vice versâ*.

In all cases of severe valgus I would suggest that the treatment be divided into two stages, as in varus, viz.: 1st, convert the talipes valgus into the equino-valgus position; and 2ndly, when you have got so far, divide the tendo achillis as in varus, and bring the foot in a short time into normal position.

Stromeyer's shoe is also a useful appliance, and it can be used for varus or valgus by just altering the screw and pad in the footboard.

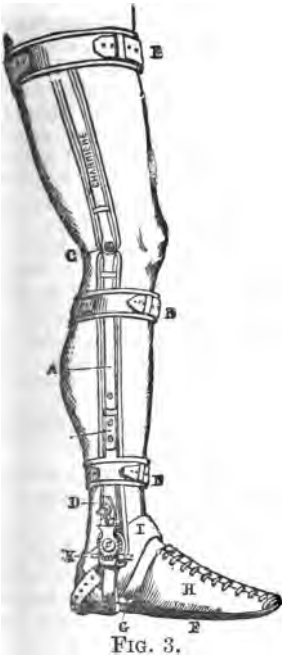


FIG. 3.

A very inexpensive and simple apparatus I am in the habit of using for cases of the kind, particularly in hospital practice—viz., an instrument precisely the same as I finish up in cases of *varus*; it consists in a boot laced up from the toes above the ankle, and a light set of irons attached to this boot, passing up at each side of the leg, and attached to the under surface of the heel, a joint in the irons corresponding to the ankle and knee, and strapped and buckled at the ankle, centre of leg, and above the knee, very much after the form of Fig. 3. This appliance I order the patient's friends to keep applied for three months, or even longer, after the tendons have been divided. In slight cases, where there is no operation required, and a slight tendency to flat foot, I order a boot with an arched sole, to induce, as it were, the plantar fascia to assume artificially the arched appearance, which, after some time, becomes permanent. In the slightest grade of valgus strips of adhesive plaster may be used with advantage, as also the gutta-percha plan, as recommended under the treatment of talipes varus. Paraffin bandages are very much used by some surgeons, also Mr. Barwell's plan, with vulcanised india-rubber bands and adhesive plaster, as also mentioned when considering the treatment of varus. Any instrument, however applied, must be worn day and night, as the permanent cure greatly depends on this point. Charriere gives two very useful appliances in his Catalogue (Figs. 2 and 3), which are very well

adapted for talipes valgus and talipes equinus; they are provided with cogwheels, and a key, and ratchet-joints, and can be turned to suit nearly any distortion of the foot.

Talipes Calcaneus.—This form of club-foot is of rather rare occurrence, and not often met with; it consists, however, in a depression of the os calcis and lengthening or elongation of the tendo achillis, with contraction of the anterior muscles of the leg, viz., extensor longus digitorum, tibialis anticus, and extensor proprius pollicis; the plantar fascia is considerably contracted, rendering the sole of the foot concave; in some cases the person affected walks on the heel, and in advanced cases the toes are drawn directly upwards, and do not touch the ground, as in Fig. 4. This deformity is met with both as a congenital and non-congenital affection, and as the appearances and external characters of each form differ considerably, a separate description of the two varieties might be judicious.—*Medical Press and Circular, Dec.* 3, 1873, *p.* 493.

FIG. 4.—*Talipes Calcaneus.*

54.—ON HOSPITALISM, AND THE CAUSES WHICH DETERMINE THE MORTALITY AFTER AMPUTATIONS.

By JOHN ERIC ERICHSEN, Esq., Senior Surgeon to University College Hospital, and Holme Professor of Clinical Surgery in University College, London.

[There is no doubt, whatever, that the septic poison, which is the cause of hospital gangrene, septicæmia, pyæmia, and erysipelas, when once generated, is capable of transmission through the medium of the atmosphere. That it may be generated in the air by overcrowding is equally certain.]

It would, in fact, appear that the air of a ward is capable of oxidising and destroying or absorbing a certain amount of morbid emanations from the contained patients; but if these emanations are developed too rapidly or too abundantly the air becomes overcharged with septic matters, and then all the ill-effects of overcrowding at once develop themselves. The contamination of the air of a ward may thus take place even though the actual number of patients lying in it be below what it is constructed to hold, as readily as by the introduction of

one single patient beyond the number that the ward is calculated to accommodate with safety. Thus it would be more correct to say that the evil effects of overcrowding, and the special form of disease it will occasion, are rather dependent on the nature than on the actual number of the cases that are contained in any given ward or building at one time. There is no evidence that an accumulation of unwounded patients to any extent can develop hospital gangrene or pyæmia, whatever other disease may thus be generated. Pyæmia could not have been generated in the Black Hole of Calcutta; but a very trifling excess of the number of open suppurating wounds beyond what any given ward is capable of holding with safety constitutes overcrowding in a surgical sense, and will invariably generate septic disease.

It is the nature of the cases, and not their number merely, that constitutes the danger of the development of surgical septic disease by so-called overcrowding. Doubtless epidemic influences favour the development of these septic outbreaks that are directly produced by overcrowding. In what these epidemic influences consist I know not. So far as my experience and observation go they are connected with very different meteorological conditions. In wet and in dry weather, in heat and cold, during prevalent easterly and westerly winds, they equally manifest themselves. The existence of these epidemic conditions, in addition to the influence of overcrowding, is evidenced by the admission from out of doors of erysipelatous cases and allied diseases, or by their appearance amongst the out-patients in unusual numbers. But the influence of different meteorological conditions on the development of epidemic septic diseases has yet to be worked out.

Now what is this condition of contamination in hospital air that causes the development of septic disease in a ward, the cubic and floor space of which are sufficient, and which is kept scrupulously clean? That is the next point for inquiry. In the well-known, I may say celebrated, lecture by Prof. Tyndall on "Dust and Disease," that distinguished philosopher points out the important fact that the atmosphere under all circumstances contains floating particles of organic matter; that the motes which dance in the sunbeam are of this character. He states that the air of our London rooms is loaded with this organic dust, nor is the country air free from the pollution; and he goes on to observe that, however much it may disguise itself from ordinary observation, "a powerful beam of electric light causes the air in which the dust is suspended to appear as a semi-solid rather than as a gas. Nobody could in the first instance, without repugnance, place the mouth at the illuminated focus of the electric beam and inhale the dirt revealed there. Nor is the disgust abolished by the reflection,

that although we do not see the nastiness, we are churning it in our lungs every hour and minute of our lives. There is no respite to this contact with dirt; and the wonder is, not that we should from time to time suffer from its presence, but that so small a portion of it would appear to be deadly to man." Watts states that a minute quantity of organic matter, one grain in 200,000, is found in the present mountain air. This increases in the country at a lower level, and in large towns reaches the condition stated by Professor Tyndall in the above extract. Even in sea air, according to Dr. Rattray, in the last volume of the Medical-Chirurgical Transactions, it is not absent. This organic matter is the result, in all cases, of vegetable and animal emanations, and it necessarily varies in its composition according to the source from which it is derived. Rattray, in a most interesting paper in the volume referred to, gives an analysis of ship air, and after pointing out a variety of impurities of a gaseous character, states that the more solid impurities are of animal, vegetable, and mineral origin, from the skin, lungs, &c., of the crew; from the ship, as minute particles of wood, paint, whitewash, leather, &c.; from the bilge, containing occasionally minute microscopical animal and vegetable organisms; from the stores, particles of bread, cotton, wool, &c., and others which I need not mention. He states that it is the volatile organic matters thrown off by the skin which give the ship air its well-known and often nauseous smell. Dr. Parkes, in his admirable work on Practical Hygiene, gives the following account of hospital air. At page 88, he says: "I have examined the air of various barracks and military hospitals, and have detected large quantities of epithelium from the skin, and, perhaps, the mouth." At page 99, he says: "The most important class of diseases produced by impurities in the atmosphere is certainly caused by the presence of organic matters floating in the air, since under this heading come all the specific diseases. The exact condition of this organic matter is unknown; whether it is in the form of impalpable particles, or of moist or dry epithelium and pus-cells, is a point for further inquiry." And again, at page 106, he says: "The air of a sick ward, containing as it does an immense quantity of organic matter, is well-known to be most injurious. At a certain point of impurity erysipelas and hospital gangrene appear. The occurrence of either disease is, in fact, a condemnation of the sanitary condition of the ward." Again, at page 100, he states that "erysipelas and hospital gangrene in surgical wards are often carried by dirty sponges, dressings, &c. Another mode of transference is by the passage into the atmosphere of disintegrated pus-cells and putrefying organic particles, and hence the great effect of free ventilation in military ophthalmia, in

erysipelas, and hospital gangrene." Lastly Dr. Douglas Cunningham, of Calcutta, has recently found that the atmospheric dust largely consists of spores of fungi, and that the majority of these are living and capable of growth and development; and that bacterial matter exists also in dry dust, so that when this is added to putrescible fluids a rapid development of fungi and bacteria occurs. Here then we find, on the combined authority of able and recent observers, abundant evidence not only of the existence in the atmosphere of large quantities of suspended organic matters, but of animal *débris* and exfoliations, and of other animal particles, capable under favourable circumstances of generating septic diseases.

We are still in complete ignorance as to the precise nature of the septic poison that produces those various forms of disease that we recognise as being due to its influence. We know it, in fact, by its effects, but are ignorant of what it essentially consists. That there are different forms of septic disease appears more than probable. We find that the different varieties of septic diseases are not interchangeable, but are as distinct in themselves, in the symptoms they present, in the course they run, in the pathological conditions that are found, as any of the other zymotic diseases. In whatever way originating and in whatever it may essentially consist, there can be little doubt that this septic virus is communicable from patient to patient through the medium of the organic particles of various kinds with which the atmosphere of any crowded building, be it hospital, or barrack, or man-of-war, is invariably charged. And when we reflect on the exceedingly minute, infinitesimal, in fact inappreciable, quantity of any animal virus, as that of cow-pox, small-pox, or syphilis, that is needed to communicate its own disease when applied to a fitting soil, we can easily understand how the virus of the septic diseases that occur in surgical wards may be transmitted from wound to wound, even on so slight a vehicle as atmospheric dust.

There are four of these septic diseases universally recognised by surgeons—viz., hospital gangrene, septicæmia, pyæmia, and erysipelas. Let us very briefly study the mode of development of them.

With regard to hospital gangrene I need say but little. It is universally recognised by the concurrent testimony of military surgeons that this disease originates in the first instance as a direct consequence of overcrowding of the wounded in hospitals that are insufficiently ventilated. The experience of the Franco-German war added confirmation, if any were needed, to this view of the origin of the pestilence, which, however, when once generated, is capable of indefinite propagation by

contact through the medium of the fingers, instruments, sponges, and surgical appliances. The occurrence of hospital gangrene in civil hospitals is now, fortunately, extremely rare, and its development in such institutions is of itself an evidence that the sanitary condition of the building is, for the time at least, in a bad state. The repeated occurrence of hospital gangrene in a civil hospital would undoubtedly be discreditable to those who had the management of its sanitary arrangements. In University College Hospital we have now had no outbreak of hospital gangrene for more than tweny years, and I trust never again to see it here as having been developed within the walls of the hospital. It is altogether a preventable disease, and ought never to occur in any institution that is conducted on sanitary principles. The few cases that we have had of late years have been brought into the hospital from without, and, curiously enough, last summer two cases came to us, of a mild form certainly, from a convalescent institution.

Septicæmia is not of very common occurrence, but yet there is a certain percentage of deaths occurring from it. In this hospital we have about one death in the year from this cause. Its influence on the general mortality after operations is therefore but trifling, but its existence is an indication of a septic influence, and I believe that a certain number of cases of low, irritative fever following operations and injuries partake of a septicæmic type. In fact, the gradation from ordinary traumatic fever into septicæmia is easy, and I believe not very unfrequent. Surgeons themselves suffer at times from small whiffs of this poison. Who that has been long connected with a hospital has not at times, after the examination of a sloughing cancer, or some other horribly putrescent and foul disease, felt feverish, depressed, prostrated for some hours, or a day or two, conscious of the absorption of a poisonous effluvium, which after a period of febrile depression eliminates itself from the system by an attack of offensive diarrhœa, the eruption of a pustule, or perhaps by some more distinctly localised inflammatory action, as tonsillitis, or a boil? The cause of septicæmia appears, however, to me to be somewhat obscure. It does not seem to be distinctly connected with overcrowding, but rather with the development of putrescent discharges from unhealthy or malignant ulcers. The offensive discharge from ulcerated cancer uteri is supposed by some to tend very specially to its production, and it has been a cause of death after ovariotomy when the operation has been practised in the same building in which a woman suffering from that disease is lying.

But the two most important of the septic diseases are undoubtedly pyæmia and erysipelas. These two diseases are commonly looked upon as being more or less allied, and they

may be so in so far as their etiology and those epidemic influences that predispose to their occurrence are concerned, but in all other respects they differ widely.

Acute pyæmia is essentially a hospital disease. As has already been stated in the last lecture, it seldom occurs except in hospital practice; and perhaps the best proof that we have of the rarity of the occurrence of acute pyæmia out of hospital exists in the rarity of the admission of a pyæmic patient into a hospital from without whilst labouring under the disease. Almost every case of pyæmia that I have seen in hospital practice has originated within the building itself. By reference to Table B it will be seen that the amount of pyæmia varies very greatly in different London hospitals, which is an additional proof of its being dependent for its origin on conditions that are more rife in some institutions than in others. So far as University College Hospital is concerned, there appears to be at present a tolerably uniform amount of it. In three years, from July, 1870, to July, 1873, we had twenty-three cases: nine in connexion with amputations, fourteen with other operations and injuries. Of these twenty-three cases, four occurred in the latter half of 1870, eight in 1871, eight in 1872, and three in the first half of 1873. As to the mode of its development, there can be no doubt that it is the result of the exposure of wounds to an atmosphere overcharged with organic matter emanating from other patients with suppurating wounds. What I stated in the last lecture about the infection of the military hospitals with pyæmia during the Franco-German war, and the comparative freedom of the hut hospitals from this disease, points clearly to the cause. I look upon pyæmia, when of traumatic origin, as being primarily the local septic impregnation of a wound by atmospheric matter in a condition capable of developing changes of such a character in the wound that its fluids decompose, its surface becomes foul and sloughing, and the veins leading from it become plugged with soft clots, putrid and easily decomposable. The constitutional symptoms of this dread disease—the prostrating rigors, the profuse transudations from skin and lung saturating the bedclothes and contaminating the air around with a faint, sickly odour, the high temperature, the extreme mental depression—are all consequent on the entry into the circulation of the septic virus deposited from air upon wounds, thence absorbed into the veins, and thence transmitted through the system.

Pyæmia stands next to hospital gangrene amongst septic diseases of local origin. It is less marked in its local phenomena; it is far more developed in its constitutional symptoms, but though less marked locally it is most distinctly characterised. Mr. Beck, who has made the post-mortem examinations of the

twenty-one fatal cases of pyæmia that have occurred in University College Hospital during the past three years with a degree of minuteness and care that leave nothing unnoted, states that in every case except one—a case of necrosis of the tibia, in which there was no open wound until a few hours before death—distinct local evidences were found. In fourteen cases the local origin of the disease was evidenced by broken-down and putrid clots in the veins leading from the part, and in six there were foul sloughing wounds. It is a remarkable fact that acute pyæmia never appears to occur in single isolated cases, but invariably in groups of two or three, not necessarily absolutely contemporaneous, but only separated by short intervals of time.

Is pyæmia contagious? The French surgeons generally believe that it is highly so. I have never seen an unequivocal instance of it. This, however, may be accounted for by the fact that whenever a case of pyæmia has developed itself in this hospital immediate steps have been taken to guard against contagion by isolating the patient and disinfecting the ward. We act, in fact, as if its contagion were proved, although we may not in reality be in possession of this proof. There certainly appears to be an epidemic influence that favours its development. It is often coincident, though not invariably so, with outbreaks of erysipelas both in the hospital itself and out of doors. I have already said that it is necessary for the surgeon to consider the nature rather than the number of the cases contained within a surgical ward. This is well exemplified by an outbreak of pyæmia that took place in Ward 1 in January, 1871. In the early part of the month this ward contained a rather large number of severe wounds, including a sloughing cancer in the groin, an amputation of the leg, a compound fracture of the fibula with extensive laceration of the soft parts, a compound fracture of the femur, and one of the tibia. Pyæmia occurred in the case of amputation of the leg on the 19th of January, in the compound fracture of the fibula on the third of February, in the compound fracture of the femur on the 13th, and all these patients died. At no time, however, was the ward overcrowded as to the number of patients, two out of fourteen beds being empty the whole time, so that each patient had a cubic space of no less than 1800 feet of air. But the ward contained for two months an average of seven or eight open wounds, of which five were always severe, such as amputations or compound fractures. During this period there was only one case of erysipelas, and the hospital generally was free from that disease. Another instance of a similar kind occurred in December, 1872. At this period Ward 1 became again crowded with severe wounds,

there being an average of nine open wounds, seven or eight of which were severe, including three amputations, and an extensive necrosis of the tibia. One of the amputation cases had pyæmia and recovered, the only instance of recovery from pyæmia during the last three years and a half. Another had pyæmia and died. On this occasion erysipelas broke out in three cases. In both these outbreaks the weather was cold.

Now in erysipelas we find various influences tend to develop the disease. Contagion, overcrowding, and epidemic influences all produce their effect. About the contagion of erysipelas there can be no question. I could adduce many instances of it, but one especially occurs to me. Though it happened many years ago, it made a deep and lasting impression upon my mind. I will read an extract from the notes I took at the time. On the 17th of January, 1851, a case of phlegmonous erysipelas of the leg was accidentally brought into Ward 1. As soon as the nature of the disease was discovered the patient was removed to the erysipelas ward at the top of the building, having only remained in Ward 1 for about two hours. At this time Ward 1 was perfectly healthy, but in a few days after, a patient lying in the next bed to that into which the erysipelatous patient had been taken, and who had been operated on for necrosis of the ilium, was seized with erysipelas. On the 22nd I performed five operations on patients who were in this ward. Of these cases three were attacked by erysipelas on the 24th—viz., a case of necrosis of the tibia, one of partial amputation of the foot, and one of encysted tumour of the scalp. All these patients died. On the 24th a patient was operated on by me for strangulated femoral hernia in this ward. He was seized with symptoms of low peritonitis, doubtless of an erysipelatous character, and died on the 31st. No case of pyæmia showed itself, but all the patients in the ward who were not attacked by erysipelas had diarrhœa and severe gastro-intestinal irritation. Here were a series of lamentable catastrophes doubtless directly introduced by the accidental intrusion of an erysipelatous patient into an operation ward, the disease being then propagated by contagion, though it only remained for two hours in the ward. That erysipelas, however, may be developed in other ways there can be little question. I believe that by no means an uncommon cause of infection occurs from dissecting students. I have several times seen cases, both in hospital and private practice, distinctly referable to contamination from this cause, and I think that no student, during the time that he is engaged in dissections, should be allowed to act as a dresser in a hospital. There is a remarkable resemblance, if not an actual similarity, between puerperal peritonitis and erysipelatous inflammation

of the peritoneum following operations on the organs contained in that cavity. The influence of contamination in the way I mention is well illustrated by what occurred some years ago at the Vienna Hospital, and is described by Dr. Braidwood in his work on Pyæmia. Prior to 1846 the number of deaths amongst lying-in women in this hospital was 1 in 10. In order to check this frightful mortality, Dr. Semelweiss required the students who attended these women to avoid making autopsies, and obliged them to wash their hands in a solution of chlorine before and after making vaginal examinations. The result was that the mortality in 1848 had fallen to 1 in 74.

That erysipelas is often of epidemic origin there can be no question, but the influence of any epidemic is immensely increased by unhealthy conditions of a ward from overcrowding. Of this I will give you one illustration. In the middle of November, 1872, ward No. 1 was filled by a very severe set of cases. There was one patient who had had both legs amputated below the knees; another whose leg had been amputated through the knee; another who had a compound fracture into the ankle-joint; one with extensive necrosis of the tibia; and another with extensive laceration of the forearm. And this condition of things was kept up during three primary operations—one of the foot and the others of the thigh; these cases being admitted in December, but whilst the preceding ones were under treatment. There was no erysipelas in the ward until Nov. 15th. On that day the patient with compound fracture into the ankle was attacked. He recovered, and the erysipelas had left him by the 21st. On this day the patient with necrosis of the tibia had the sequestrum removed: he was a feeble old man; he was attacked by erysipelas on the 1st, was removed to the erysipelas ward, and died on the 7th of December. On the 4th of December the patient with the scalp wound was attacked, and was at once removed to the erysipelatous ward. During this period—namely, Dec. 7th and the 22nd—two of the other patients, the one with an amputation through the knee-joint and another, were seized with pyæmia. Only one out of the twenty-three patients with acute traumatic pyæmia recovered. During the same period two of my patients in Ward 5, both suffering from chronic wounds, one of fistula and another with a sinus in the thigh, were attacked by erysipelas. To them it is possible that the disease was conveyed by the house-surgeon or dressers. But as other cases of erysipelas occurred in the hospital at the same time, and one was admitted from out of doors, it is probable that an epidemic influence existed.—*Lancet, Jan.* 31, 1874, *p.* 151.

55.—ON THE USE OF RESIN-CLOTH, IN THE PLACE OF CERE-CLOTH, IN THE TREATMENT OF WOUNDS.

By EDWARD LUND, Esq., Surgeon to the Manchester Royal Infirmary.

[The carbolised gauze employed by Professor Lister is expensive, which is a matter of importance in hospital practice. There is, moreover, a difficulty in its preparation.]

In the paper in which he first described this dressing in the British Medical Journal (January 1871, p. 30), Professor Lister refers to a very curious property possessed by carbolic acid—namely, that it can be combined with resin, and resinous matters generally, with great facility; and, when so blended, nearly all its irritating acrid properties are neutralised, while the compound so formed retains the power of evolving an antiseptic vapour at the temperature of the body. It is well known that to touch the mucous membrane, or even the skin of the lips, with a fragment of pure carbolic acid, occasions pain and excoriation, in some subjects more than in others; but when we experiment in the same way, on a mixture of the acid with five parts of resin, we find all this acridity destroyed; the acid is still present, but it is stored up and rendered harmless by this new combination. Having verified this fact by numerous observations, I came to the conclusion that we have here a method of forming easily, without the aid of heat, an application for antiseptic purposes in surgical practice of great value. All that is required to carry out the idea is to saturate very thin calico gauze with such a mixture of resin and acid, dissolved in methylated spirit, press it powerfully, spread it out to dry quickly, so that it may become quite porous and absorbent, and it is ready for use. Yet the resin and acid, thus left upon the threads of the calico after the complete evaporation of the spirit, would be too brittle and adhesive to the skin for a wound-cover and air-filter if something else were not added to impart flexibility. The substance which I employ for this purpose is castor-oil, because it is the only fixed oil easily accessible, which is quite miscible with spirits of wine. All samples of castor-oil, by reason of adulteration, do not show this property of perfect solubility in alcohol, as stated by Pereira (Materia Medica and Therapeutics, 1865, p. 251); and, therefore, I am content to use the oil, if it will unite perfectly with twice its bulk of rectified spirit. The exact composition of the solution is as follows:—Carbolic acid crystals, melted, two fluid-ounces; castor-oil, two fluid-ounces: purified resin, sixteen ounces by weight; methylated spirit, forty fluid-ounces: mix. To dissolve these ingredients easily, we must add them together in a certain order. To the

resin, liquefied by heat and removed from the fire, add one-third part of the spirit; when these are well mixed, put in another third of the spirit, in which the oil has been previously dissolved; and, lastly, the acid in the remaining portion of the spirit must be slowly added to complete the mixture. The whole must be agitated until all the constituents are thoroughly incorporated, and afterwards passed through a muslin filter to get rid of any extraneous matters. If this plan be not adopted, the resin will concrete into a mass at the bottom of the vessel, and it will be extremely difficult afterwards to get it perfectly mixed. When thus prepared, the solution is of a dark colour, clear and free from any deposit; and it can be kept unchanged, in a well corked bottle for a long time. It is, in fact, a kind of thick varnish. To make the resin-cloth, as I term it, for the sake of distinguishing it from the cere-cloth dressing for wounds, which I described in a paper read at the Leeds meeting of the British Medical Association in 1869, it is needful to select a very thin, cheap, porous calico, or calico-muslin, known in the trade as "mulls," which costs at wholesale price about four shillings per piece of twenty yards. This, divided into strips, each about nine inches wide and six yards long, is reduced to a convenient form for general use. The calico should be unbleached and free from stiffening, and each of the strips should be carefully folded up, so as just to lie flatly in the press, as I am about to explain.

An ordinary square tincture press may be used to press the cloth, or such a press as is sold for copying letters, to which a tin box has been adapted, so that the plates of the press can work in it; and into this box the folded calico is placed, the solution being poured over each successive layer, so as to wet perfectly every part of it. There should be an aperture at the bottom of the box, with a tap by which the superfluous fluid can be removed, collected, and used a second time. The press being brought into action, the pile of calico should be squeezed as dry as possible, all the fluid drained off, the resin-cloth taken out, laid over a few lines of string in a warm room with a good ventilation; and in an hour or two, when all traces of smell of the methylated spirit have departed, the cloth may be rolled up and kept in tin cases ready for use.

It is difficult for me to state the exact cost of resin-cloth made by this process, for I have not yet bought the materials for its preparation at such prices as could be had if it were to be manufactured in large quantities; but, allowing for some slight reduction, where six or eight twenty-yard pieces of calico are bought at a time, and the solution made by the gallon, I find it comes to a fraction less than threepence per yard of average width of 44 inches. In using it as a dressing

for wounds, I deal with it precisely as I would do with Mr. Lister's antiseptic gauze, for which it must be taken as a cheap and ready substitute. I generally apply ten folds of it over the face of a wound, as in an amputation, and perhaps six folds higher up the limb for some distance, and I cover it with the macintosh hat-lining, so as to distribute the serous discharges through the breadth of the resin-cloth thus covered. I have never found it to irritate the skin in any degree beyond what the oiled silk (protective), liberally used, could control; except once, when in the hurry of preparing the resin-cloth, I had neglected to dry it thoroughly, and it was applied, still moist with the methylated spirit, the naphtha in it seeming to be the chief cause of the skin-irritation. But if this precaution be observed, I believe this resin-cloth will be found a very useful addendum to our means of treating wounds and abscesses on antiseptic principles.—*Brit. Med. Journal, Dec.* 6, 1873, *p.* 654.

56.—ON A NEW FORM OF FIXED BANDAGE.

By W. W. WAGSTAFFE, Esq., Assistant Surgeon and Lecturer on Anatomy at St. Thomas's Hospital.

In the course of last year my attention was drawn, by Mr. West's address upon French Surgery delivered at the Midland Medical Society, to the silica bandages used in the Paris hospitals in place of gypsum and starch, and, being then Resident Assistant-Surgeon at St. Thomas's Hospital, and able to find frequent opportunities for testing the value of new appliances, I was desirous of trying this. However, I found that no such preparation of silica for bandages had been brought into use in England, and upon inquiring of Mr. West himself, found he had been unable to succeed with the preparations obtained in England. The French directions, too, for its preparation were so useless to a surgeon that I despaired of being able to find anything at all satisfactory. However, I was fortunate enough to obtain, through my friend Mr. A. H. Smee, a small quantity of colloidal silicate of soda, and with this succeeded in making an exceedingly firm bandage. This bandage was put on over the knee in a case of knee-joint disease in July, 1873, and was removed only in November, after four months' wear, and at the end of that time it was perfectly strong, clean, and sound. This bandage was a single length arranged in three successive layers, each being covered with the silicate; and the whole quantity of the silicate used was four ounces.

The silicates of potash and soda, dissolved in an excess of caustic alkali, are now prepared in large quantity in the manu-

facture of soap, and can be easily obtained at a very moderate cost. Messrs. Hopkins and Williams, of 16, Cross Street, Hatton Garden, have supplied it in large quantities at 4d. a pound, and in small quantities at 6d. a pound, and it can be obtained, though inferior in character, at 2d. and 3d.,—so that it is not an expensive material. In fact, it is about half or a quarter the price of the thick gum solution used for similar bandages, and being firmer, does not require to be used in much more than half the quantity.

The plan which I have adopted has been to encase the joint or fractured limb in cotton wool, lint, or a thin flannel roller; then over this to apply a common bandage, dry; over this to paint the silica by means of a brush or sponge; and to repeat the bandage and silica so that two or three layers of each exist. As soon as the last layer is dry, another coating of silicate is to be put on, so as to give an even surface.

The limb is to be left exposed to the air for about half an hour, but there is no fear of any of the silicate coming off after the first few minutes, and after half an hour or less (varying with the temperature) the bandage is firm enough to prevent movement. However, the bandage continues to harden for about two or three days, at the end of which time it should be quite firm; but it is usually firm enough in a few hours to insure immobility of a limb. It is sometimes advisable, as where great strength is required, to coat the splint after a day or so with fresh silicate; but where such strength is wished for, it is better to adopt a modification to which I shall refer directly. The fixed bandage, as now completed, does not contract in drying, and does not produce the inconvenience which is often seen with gum bandages—that of contracting at the margins and cutting into the skin. It forms a fixed splint of great lightness, cleanliness, and strength, and of less expense than gum. With children I generally take the precaution of varnishing over the surface with spirit varnish after a couple of days, so as to keep the bandage from the wet to which it may be exposed. The solubility of the silicate in water is of advantage, for it renders the removal of the bandage easy. Without taking the precaution of moistening it, I have seen a strong pair of scissors broken in removing one of these silica bandages from the knee.

The advantages, then, which the silica bandage possesses over the gum bandage, are—(*a*) much greater rapidity in drying, (*b*) greater cleanliness, (*c*) probably greater strength, and (*d*) greater cheapness. Its advantages as compared with the starch bandage are (*a*) its greater strength, (*b*) its greater rapidity in drying, (*c*) its greater cheapness, and (*d*) its freedom from offensiveness after a time. Compared with plaster of Paris, it

is (*a*) lighter, (*b*) cleaner, but it does not set so quickly or afford quite so firm a support at first.

There is a modification to which I have alluded above as securing greater strength in this bandage. Mr. W. C. Elliott, of this Hospital, has suggested the mixing of whitening with the silicate. As much whitening is mixed with the silicate as to make a fluid of the consistence of batter. This is used in the same way as the pure silicate, and the results obtained have proved very satisfactory. In a case of dislocation of the knee from injury he adapted a fixed bandage in the following manner:—A thin layer of cotton-wool was spread evenly over the joint, and retained by a bandage; over this strips of brown paper two inches wide, and plastered on both sides with the mixture of sodium silicate and whitening, were placed in the same manner as in strapping a knee, and the whole was plastered over with the mixture, so as to procure an even surface. This bandage remained on for eight weeks, and at the end of that time it was removed; it was still perfectly firm. The patient was able to walk about comfortably with this bandage on.

In removing the silicate and whitening bandage some difficulty may be experienced, owing to its great hardness, but, if care be taken not to have too great a thickness of the coverings, this difficulty may be avoided.

We have therefore in this silicate bandage a means for providing immobility of joints and fractures which far surpasses in convenience, lightness, cleanliness, firmness, and cheapness any of the means hitherto made use of. If a limb be covered with cotton-wool, lint, wool, a worsted stocking, or any other soft protecting material, a surgeon may make use of strips of linen, bandage, or paper saturated with silicate of soda mixed or not with a salt of lime, such as chalk, whitening, or plaster of Paris, and procure a rapidly setting and extremely firm splint, the expense of which is reduced to a minimum.

I have used it now frequently in cases of fracture without swelling, or before swelling has come on, and with most beneficial results. The silicate of potash is, in my experience, inferior to the silicate of soda, and it offers no special advantage in any respect.

In the mixture of silicate of soda and whitening it appears, from experiments which I have made, that a partial decomposition occurs, and a lime silicate is formed, which is of very great strength. But the great hardness of this preparation proves sometimes objectionable, and I have now made use of a mixture of starch and silicate, with the effect of procuring an extremely firm, but slightly yielding, bandage. It seems that the addition of the starch thickens the preparation only,

and allows of its being put on more as a paste; and as the mixture when dry is slightly yielding, it does not press so much upon bony prominences, and therefore is less likely to cause injury than the unyielding lime silicate. Upon the whole, I am inclined to prefer the simple silicate for the majority of cases, as requiring less preparation; but, where special strength and immobility are required, I give the preference to the mixture of silicate and starch or silicate and whitening.

Another plan which I have adopted recently is to moisten the bandage with a saturated solution of calcium chloride, and apply the sodium silicate to this in the same manner as with the dry bandage. The result of this is to form a very hard bandage, which sets quickly. This necessitates, however, a double process, and is therefore not quite so ready of application as the pure silicate alone. In all these applications it is advisable to use the best and densest preparations of the silicate. Considering the readiness with which it can be applied, its cleanliness, and the smallness of its cost, I cannot but think this preparation is destined to supersede the materials at present used for the purpose of making fixed bandages.—*Medical Times and Gazette, March* 7, 1874, *p.* 263.

57.—REMARKS ON THE PRODUCTION OF SLEEP DURING SURGICAL OPERATIONS.

By Joseph T. Clover, Esq.

[There never has been a period when the opinion of surgeons as to the relative value of the different anæsthetics was more unsettled than it is at present. No doubt while there is danger in every kind of anæsthesia, there are conditions of safety, if we only knew them, that would make all the anæsthetics perfectly free from danger.]

When ether or chloroform is inhaled, the throat is stimulated; coughing and movements of deglutition commonly occur; vomiting even is sometimes directly excited, if the vapour be very strong. The act of swallowing is usually performed well enough; but, if the anæsthetic have produced sufficient effect upon the patient to interfere with reflex movements, then the diglutition may be delayed at the moment when the epiglottis covers the larynx. Raising the chin, and pulling it as far as possible away from the sternum, is usually sufficient to obviate this source of obstruction. If any one will try the experiment of attempting to swallow with his chin considerably elevated, he will find that he cannot do so without lowering it. When chloroform or ether vapour of greater pungency is given, there

may be a direct spasm of the glottis produced. A similar spasmodic closure often occurs when nerves are pinched or divided in the course of an operation. It commonly occurs when a ligature is applied to a hemorrhoid.

I have never found it necessary to take hold of the tongue with forceps; but I advise that they should be kept ready, for it may be that the greater pungency of the vapour, when applied freely, causes an obstruction which raising the chin will not remove; and in such a case we must not be afraid of hurting the tongue.

Ether.—I was a junior student at University College, and saw Liston perform the first capital operation under ether that was done in this country. Although it continued to be used in certain cases in the operating theatre, there were objections to it which even now are but partially removed. The struggling against it, and the strange things patients said and did under its influence and whilst recovering, made us receive chloroform gladly, as a more convenient and less disagreeable agent. It was given with much freedom, until a death occurred at Newcastle in the practice of Sir John Fife. Of late, chloroform accidents appear to have increased, and thus medical men have very properly been stimulated to seek for a safer anæsthetic. Ether is undoubtedly safer; that is to say life is not so quickly destroyed by it. But, nevertheless, I have seen a strong dog killed in sixteen seconds by being made to breathe pure ether through a tracheal opening. In the case of death at the West London Hospital after inhaling first chloroform and then ether, the patient had recovered from the chloroform so far as to have a fair pulse; but it ceased to beat very soon after the application of ether by means of a sponge made warm with water. The chloroform had, I suppose, sufficiently narcotised the patient to prevent the usual obstruction to strong vapours by closure of the glottis. If we are prepared to resist the remonstrances and struggling of the patient, ether may be poured upon a hollow sponge or towel folded into a cone, or upon a cone of felt covered with oiled silk, and held over his face. The patient will resist breathing, at first voluntarily, and afterwards more or less spasm of the glottis will be produced. The blood will be imperfectly oxygenated; and insensibility will be produced in a short time, partly from deprivation of oxygen, and partly from the action of ether. Less ether enters the tissues of the body in this way of giving it; and, if the operation last only two or three minutes, the recovery of consciousness will be speedy. Headache is not rarely complained of. Sickness is about as frequent as after a moderate use of chloroform. The patient sometimes remembers the choking sensation, and will not inhale it again if required.

The flow of saliva is considerable; and the larger air-tubes are often obstructed by fluid, which makes a loud moist rhonchus; but it does not appear to be of much consequence, as it goes off afterwards.

I think most English surgeons are in favour of giving the ether slowly for about a minute or so at first; and then, when some degree of anæsthesia has come on, to give the vapour strongly. I am decidedly in favour of this plan, and have sometimes succeeded in getting a patient to sleep without any struggling. The most pleasant way is to give nitrous oxide till consciousness is lost, and then administer the ether of moderate strength. I should be afraid of giving *pure* ether by means of a large warmed sponge to a patient already well narcotised, for reasons already mentioned.

I have given ether a good many times by means of a bag, smaller than the one in my chloroform apparatus. The bag was regularly supplied with air, which passed over a large surface of ether, kept at the temperature of about 68 degrees, at the rate of about five-hundred inches per minute. I have for the present put this aside, and make use of what I term a double-current inhaler for ether. The object of this instrument is to effect a mixture of ether and air of the strength required. The internal arrangement is on the plan of an ordinary still, supposing the worm to be covered with calico for holding the ether. The ether is kept at a comparatively uniform temperature, the coldness produced by the evaporation of the ether during inspiration being balanced by the condensation of ether and watery vapour during expiration. The valves are so placed that the inspired air passes over the ether, and the expired air passes through the condenser. The ether in the upper part of the vessel being kept several degrees lower than the patient's breath, it is impossible for the proportion of ether-vapour to exceed a certain point. This proportion is more pungent than is usually required, and is diluted at pleasure by a sliding valve which admits air without passing over the ether. The advantages obtained are—1. The power of graduating the dose so as to avoid very much of the unpleasant pungency of ether as commonly administered; 2. Diminution of the smell of ether in the room; 3. It may be used in hot or cold weather without any water-bath, and the use of glass is avoided; 4. Economy of ether; 5. Saving of time. It can be kept ready for use, and will serve for four or five cases without adding more ether.

Directions for Use.—1. Put ether into the apparatus, after removing the inspiring valve, until the vessel is filled to the level of the bottom of the tube. About ten ounces will be required. 2. Refix valve and cap, and rotate the box slowly

on its long axis, to distribute the ether over the calico on the outside of the condenser. 3. Open the regulator, remove the three caps, and apply the tube with face-piece. 4. After using, pour out condensed vapour. It will generally be found convenient to suspend the inhaler by means of the strap passed round the neck; but as soon as the patient is asleep, if not before, it can be placed upon the couch. The condensed ether-vapour might be saved and redistilled, if the apparatus were extensively used in hospital practice. In order to prevent the escape of ether-vapour from the apparatus when not in use, I keep it in a tin box, the front and sides of which are double, so as to form a trough for water, into which the lid of the box is immersed. Of course, if the box be heated, the air inside may expand and escape in bubbles; but very little vapour is lost in this way.

Chloroform.—Next to nitrous oxide, I think chloroform is the least unpleasant anæsthetic to breathe. Patients who have previously taken chloroform are often unwilling to try nitrous oxide, because they have no objection at all to the former; but I have rarely found a patient who had once been put fully under the influence of ether, who was willing to inhale it again, unless it had been preceded by nitrous oxide.

Much has been said against chloroform, on account of the sickness which follows its use. I do not find it occur on an average more than once in seven or eight cases. The morphia or other preparations of opium, which are often prescribed after operations, will account for some of the cases of obstinate sickness. I admit that chloroform has more deaths to account for than ether has; but we must not forget that chloroform is much more frequently administered than ether, and that coroner's inquests are not held in countries where the practice of giving ether instead of chloroform is prevalent. Still there are greater dangers from chloroform than from ether, although I do not think them so insidious that every one who inhales it runs even an infinitesimal chance of losing his life.

The proportion of chloroform to the blood which will destroy life varies according to the species of animal, and in some slight degree according to the individual, but there is no animal that may not easily be killed by a full dose; and, therefore, whatever way we give the chloroform, it must be only a reasonable precaution to endeavour to avoid reaching that dose.

I have already explained how it happens that one person whose breathing is *excessive*, whilst his pulmonary circulation is *slow*, may get as much chloroform into the blood of the pulmonary veins, from air which contains a moderate quantity of chloroform, as another person, whose breathing is *feeble*

and whose pulmonary circulation is *rapid*, may get from air which contains a much larger proportion of chloroform. Therefore, in whatever way the chloroform is given, the condition of the patient's pulse, as well as his breathing, should be carefully watched all the time he is inhaling; and, when the breathing is deep or the pulse weak, the chloroform should be diminished or removed. In the administration of anæsthetics generally, but especially in giving chloroform, it is advantageous to have the stomach empty or nearly so. I like to give a teaspoonful of brandy, without water, a few minutes beforehand, but not so much as a tablespoonful. If wine be given, or if the patient must have some water with the brandy, then they should be given half-an-hour before inhaling, to allow time for their absorption. Emphysematous subjects, especially, ought to avoid inhaling after a meal, but I do not consider them bad subjects for chloroform. The majority of fatal cases have had good lungs; and, seeing that it is by free respiration that the greatest amount of chloroform enters the blood, we should expect cardiac syncope to happen less frequently in cases where the respiratory function could not be much increased by excitement. It will appear a strange opinion, but I say advisedly, that I think there would be less danger from giving chloroform to the out-patients of a consumption-hospital, than to the same number of distinguished athletes. The subjects of advanced fatty degeneration are bad subjects for any excitement or any anæsthetic, but we have no reliable means of diagnosing such a state. I believe I have given it several times when the heart was fatty. Where the disease is believed to exist, greater caution should of course be used, and it would be well to add one-sixth or one-eighth part of ether.

If I did not use any special apparatus, I would fold a piece of lint into a compress, of the size of a walnut, and tie it up; then pour upon each end of it a drachm of chloroform, so as to let it penetrate the lint without wetting all the surface; then let the patient smell at it at the distance of two inches, and gradually cover my hand and the patient's face with a towel; by warming the lint with the palm of the hand, nearly the whole of the chloroform can be evaporated slowly. It is known, by its no longer feeling cold, when the compress has become nearly dry and needs to be supplied with fresh chloroform. Not more than thirty minims at a time should be added after the first charge.

Of course, the proportion of chloroform in the atmosphere breathed is, in most of these ways, modified by circumstances: *e.g.*, by the temperature of the room, and of the chloroform at the commencement; then by the amount of cold produced by evaporation of the chloroform; by the distance the chloroform

is held from the mouth; by the temperature of the hand, and the manner of holding the chloroform.

The slowness with which a timid patient inhales causes so much delay, that the administrator is sometimes induced to augment the dose; and then, when consciousness is lost, the patient is very liable to get more of it than is safe. Not a few alarming cases have had no bad symptoms until fresh chloroform was supplied; and then, in from a quarter to half a minute, as soon as there has been time enough for the new atmosphere to tell its effect on the heart, syncope has taken place.

Chloroform is said to prevent the shock of an operation; and it certainly does so to a great extent. We do not find fainting so common as it was before chloroform was used; but I have often felt a weakness and irregularity, and sometimes a decided arrest, of the pulse at a time when extensive incisions or injury to nerves has occurred. I think that a patient should have his pupils insensible to light, or breathe stertorously, before the commencement of a severe operation; but that one or two inspirations of pure air should be taken before the anticipated shock is given.

Methylene.—The so-called methylene bichloride is a compound body, similar in many respects to a mixture of chloroform and ether and alcohol. When put into a test-tube, with the bulb of a thermometer fixed just above the level of the liquid, and boiled, the temperature rises steadily; the more volatile substance escapes until the boiling temperature of chloroform is passed. The same rise of temperature occurs when we boil a mixture of ether and chloroform and alcohol.

It may be noticed also that, as the methylene is boiled, the smell of the residue becomes more and more like that of chloroform; but this odour diminishes again as the fluid is nearly dissipated. I do not suppose it to be simply a mixture of the substances named, but I think there is sufficient chloroform in it to make it unsafe to use it with the same freedom that ether may be used.

I have notes of about one hundred cases in which I gave it. Many of my patients were very efficiently and pleasantly put to sleep by it, and awoke with signs of cheerfulness, not usual after the use of pure chloroform; but in other cases there was very undesirable excitement. In one case, the respiration and pulse ceased, and I had to resort to artificial respiration to restore the patient. After this case, I felt less confidence in giving it than chloroform, and I now very rarely give it from choice.

In administering it, I sometimes used a cone of leather lined with flannel. The pungency of the vapour, when given in this way, makes patients often resist and struggle against it, so I

more frequently gave it with my chloroform apparatus, using about sixty minims to each one thousand inches of air. I did not find sickness less frequent than after chloroform. I think it is an useful mixture, but it should certainly be used with caution, not only because it contains chloroform, but because the quantity of chloroform given off is not always in the same proportion to the more volatile ingredient. Methylene ether made by adding ether to the methylene bichloride would be liable to the same objections in a less degree.

Mixtures of Chloroform with Other Things.—I find that four to eight minims of ether and thirty minims of chloroform with one thousand inches of air mixed in my bag, answer very well. It sometimes produces more excitement than chloroform alone, but it certainly tends to make the immediate recovery from chloroform more pleasant, and keeps up a better pulse.

The addition of a fourth part of chloroform with one of ether will make the administration of ether on a towel much easier; but, inasmuch as it must be forced upon the patient more than chloroform alone, and the patients are almost sure to struggle, I have not used it very frequently, and cannot advise it.

I have used a mixture of alcohol, one part; chloroform, two parts; ether, three parts; and like it much, for cases where moderate narcotism has to be kept up a long time—*e.g.*, when caustic chloride of zinc has been applied to a cancerous growth. I have tried, in a few cases, tetrachloride of carbon, ethyliden chlorid, and most of the other anæsthetics that have been recommended; and the conclusion at which I have arrived is, that they are neither safer nor more pleasant than ether and chloroform; and, if not decidedly superior in these respects, it is better to continue the agents we are most familiar with.

Nitrous Oxide is an anæsthetic of a very different character to the preceding. I had the pleasure of showing the use of it at the Oxford meeting. Since that time, I have given it in 6,960 cases. When successfully given, the patient appears to fall asleep without any delirium or excitement; but, if the operation be one leaving much pain behind it, the patient sometimes will have a dream more or less connected with it, and then wake up rapidly and completely. It is by far the best anæsthetic for many short operations, such as the extraction of teeth, opening abscesses or boils. It answers very well in operating for strabismus. Removal of the eyeball was performed lately by Mr. Bowman for a lady, who said she had no consciousness of the operation. It is well suited for examining hysterical cases, wrenching stiff joints, and reducing luxations of recent date. I do not think it suitable for cases where it is necessary to keep the patient quiet more than three or four minutes; but, if the patient be allowed to recover consciousness after one inhalation

before another is commenced, the anæsthesia may be kept up tolerably well for half an hour. I have never kept a patient unconscious for more than between six and seven minutes; but, if quietude is not important, I have no doubt that a patient could be prevented from feeling pain for an hour. But, although this is possible, I am sure the recovery afterwards would not be satisfactory. When chloroform or ether has been given for ten or fifteen minutes, a state of anæsthesia continues for some time afterwards; but the sensibility to pain returns within two minutes of ceasing to breathe the nitrous oxide, and therefore, in such operations as tying hemorrhoids and minor amputations, which might, from the short time they take, be thought fit cases for nitrous oxide, it would be unsatisfactory to the patient on account of the after-pain.

In administering nitrous oxide, a plentiful supply of gas is essential to success. There is no fear of patients inhaling too much at first; indeed, they should be told to breathe deeply and slowly. They often disobey the latter part of the advice, and pant as if they could not breathe too rapidly. A supplemental bag was my first contrivance for preventing air from being drawn under the face-piece, and for obviating waste of the gas in cases of this kind. I have now accomplished the same object by keeping the inspiring valve open, so that a part of the expired gas is driven back through it into the bag. I find that I can prolong the sleep of nitrous oxide during operations in the mouth by having the nose-cap and mouth-piece separate, and retaining the nose-cap in its place whilst the operation goes on. The supply of gas must be kept up freely, for, if a little air be drawn in by the mouth, the patient will soon become unsteady, if not actually sensible to pain. Gas is sometimes required where, from the inflamed state of the cheek or lips, the pressure of the face-piece could not be borne. A cap, of the size of the face, formed in front of a piece of glass, and of tin and India-rubber at the sides, answers exceedingly well, but a great deal of gas must be wasted to ensure success. It is a good plan to cover the eyes during recovery, as a bright light or any moving object may cause an unpleasant dream.

If nitrous oxide should prove fatal, I think it will happen from the occurrence of syncope, from some other cause, during inhalation. Arrested breathing would soon be remedied by artificial respiration, if the heart were beating. I have, on three occasions, been obliged to resort to this means, and the recovery in all cases was satisfactory in less than three minutes.

Nitrous oxide may be used with advantage to prepare the way for giving ether; but I would not advise it before giving chloroform, because, firstly, chloroform is not very unpleasant if given gradually, and, secondly, it would be more dangerous

to give chloroform in the state of excitement from gas than without it. I have used gas preparatory for ether in two ways. 1. By means of a bag of gas, I make the patient unconscious, and then give the ether as usual; but the patients sometimes wake up enough to struggle against the ether. We should be prepared for this, otherwise the patient may remove the inhaler and remain several minutes longer in a state of excitement. 2. By using a small bag which is supplied with gas during the inhalation, and by diverting the current of gas and making the supply of gas pass over ether, as soon as the patient loses consciousness, enough ether can be got into the system this way to keep up anæsthesia for three minutes with the mouth open. It is, therefore, well suited for small operations in the mouth, such as the extraction of difficult teeth. The best time for inhaling gas is when the stomach is nearly empty. If the state of the stomach would cause nightmare in ordinary sleep, it would also in that of nitrous oxide. About once in five hundred cases, I have known the patient to pass urine whilst under gas, the explanation being, that the bladder was so full as to require a voluntary effort to retain the urine.—*British Medical Journal, Feb.* 14, 1874, *p.* 200.

ORGANS OF CIRCULATION.

58.—CASE OF ANEURISM OF THE EXTERNAL ILIAC ARTERY CURED BY PRESSURE WITH LISTER'S ABDOMINAL TOURNIQUET.

By C. G. WHEELHOUSE, Esq., Surgeon to the Leeds General Infirmary.

[The patient was a publican, but of temperate habits. There was distinct history of syphilis.]

Twelve months ago a popliteal aneurism was found in the calf of the right leg, with pain, which became aggravated by a subsequent strain, and was treated by Mr. Wheelhouse, in consultation with Mr. Newstead, by continuous compression of the femoral artery for eight hours, by means of Porter's femoral compressor, which was well borne, and proved perfectly successful, the tumour in September last being perfectly hard, about the size of a pigeon's egg, and easily felt in the popliteal space. About the middle of July last the patient felt another tumour on the right side of the iliac fossa, constantly pulsating, and very painful. It seemed to have formed gradually, there being no history of strain or other injury, but

the right leg was colder than before. The author, with Mr. Newstead and Mr. Jessop, found a large pulsating and expansile tumour in the right iliac fossa, reaching from Poupart's ligament upwards to within two inches of the umbilicus, and extending in an outward direction almost to the spine of the ilium, about the size of a small cocoa-nut, hard and firm at the lower part, and softer in the upper portion, with pulsations and dilatations synchronous with the pulse in the left femoral artery. The swelling appeared to be connected wholly with the external iliac artery, but to extend above and overlap the common iliac; and although pressure could not be made on the latter sufficient to stop the beating, it was easily controlled by pressure on the abdominal aorta just above its bifurcation. After a consultation, treatment by pressure was determined upon, and, on Sept. 27th, chloroform was administed by Dr. Barfoot, ether substituted as soon as the muscles were relaxed, and its action kept up continuously for five hours, without any untoward symptoms, twenty-five ounces of ether being used. Lister's large abdominal tourniquet was applied just over the umbilicus, and slowly screwed down until the flow of blood through the aneurism was arrested. Two slips of the instrument occurred during the first half-hour, but it was at once replaced. Pressure was commenced at 1.15 p.m. The foot was cold and a little blue at 2.30 p.m.; at 3 p.m. the tourniquet was unscrewed slightly; pulsation in the tumour returned, and it was at once reapplied. At 4 p.m. blueness had extended beyond the knee, the left leg being very cold; at 5 p.m. the right limb was blue to the groin, and the left to the knee. Pressure was then slightly relaxed; the tumour much harder, but pulsation still perceptible. At 6 p.m. both limbs were black, and body blue as far as tourniquet. The instrument was then removed gradually in fifteen minutes, a quarter turn of the handle being taken every minute. The tumour had ceased to pulsate, and was firm and hard. The limbs were wrapped in wool and tied together, and a hypodermic injection of morphia given. (During the entire operation slight pulsation could be felt in the left femoral artery.) At 7 p.m. there was slight pulsation in the aneurism, but it was firm and hard. At 10 p.m. pulsation had increased, but the tumour remained firm, and another morphia injection was given. A restless night was passed, and next day the tumour pulsated with nearly its old force, but the walls of the aneurism felt thicker and harder than before the operation, and the beating was more "lifting" and less "distensile," the author predicting that it would slowly cease altogether. Two grains of opium were ordered to keep the bowels quiet; the limbs were recovering their natural hue and temperature, and on the second morning after the operation

the tumour was again much harder, pulsation was almost imperceptible, the patient was easy, and in the evening of the same day the tumour was almost fully reconsolidated, and both limbs normal as to warmth. Three days after the operation the pulsation was found to have stopped, the tumour was hard and firm, and when examined on November 14th (forty-eight days after operation) no pulsation was found, the aneurism had contracted, was hard and firm, about the size of a cricket-ball, the limb had completely recovered, and the patient had returned to work. The author was assisted at the operation by Dr. Barfoot, house-physician, and by Mr. A. R. Dunnage, house-surgeon to the hospital. Sphygmographic tracings taken before and immediately after the operation were exhibited to the Clinical Society.—*Lancet, Dec.* 20, 1873, *p.* 880.

59.—ON THE TREATMENT OF ANEURISM OF THE ARCH OF THE AORTA BY GALVANO-PUNCTURE.

By Dr. CHARLTON BASTIAN, Physician to University College Hospital.

[Passing over the first part of this paper, we give Dr. Bastian's remarks concerning the methods of procedure in treatment by galvano-puncture.]

It has been now thoroughly established by the labours of Dr. John Duncan, Dr. Fraser, and others, that a clot can be readily obtained in blood-vessels by the action of the galvanic current. We know, however, comparatively little about the exact nature of this clot. It is described as being of dark colour, and as differing in appearance from ordinary coagulated blood. As Mr. Holmes has pointed out in his most able lectures on the surgical treatment of aneurism (Lancet, 1872, vol. ii., p. 327), the clot, which is first formed by electrolysis, is apt at times not to be persistent. Pulsation may cease in a tumour for a time after galvano-puncture, whilst in a week or ten days, it may again manifest itself—apparently because the clot first formed has subsequently melted away. But, in addition to this clot of somewhat uncertain composition and durability, formable directly by the action of the voltaic current, we have to bear in mind the fact, that even a very minute clot produced in this manner may form the nucleus around which a process of spontaneous coagulation takes place. This process of spontaneous coagulation leads to the formation of a more or less colourless clot, composed, in the main, of interlaced fibres of fibrine containing white blood-corpuscles within their meshes. This colourless clot also grows by the deposition of successive lamellæ, and the whole forms a firm, coherent, and somewhat elastic substance, admirably

adapted to take off the pressure of the blood, and to act as a thoroughly efficient buffer. Now it was a clot of this latter nature which we found more than half filling the aneurism, and there are several reasons tending to show that the clot had been recently produced—as an indirect result, in fact, of the galvano-punctures.

It seems obvious that changes took place within the aneurism, as a result of the galvano-punctures. Thus, after the first occasion, a slight *bruit*, not previously audible, was to be heard over the sac itself; there was also apparently an altered incidence of blood-pressure for a time, since the blood began to pass more readily through the valvular opening into the left carotid—the pulsation in this vessel became more distinct. After the second galvano-puncture, there was a slight diminution in the amount of pulsation over the upper end, the *bruit* there still continuing; and then there came a gradual diminution in the flow through the valvular opening of the left carotid, as judged by the diminished pulsation. After the third operation, the flow gradually ceased through the left carotid, and a very marked diminution took place in the amount of pulsation over the lower part of the swelling, where it had been previously increasing.

It seems almost certain, therefore, that the clot which we found in the aneurism had been produced as a result of the treatment to which the patient was submitted; this view being still further confirmed by the fact, that a button-like piece of the same kind of colourless material as that which constituted the larger clot, and of just the same diameter as its apex, was actually attached to the wall of the aneurism in a situation corresponding with the puncture made by the zinc-coated needle. I think there is good reason for believing, indeed, that the apex of this clot had been accidentally separated from the parietally attached portion, during the removal of the aneurism. For the laminated strata of the larger clot, were—to use a geological phrase—"conformable" with the surface of its upper portion. The clot had been formed by the successive deposition of laminæ upon its under surface; these laminæ becoming larger and larger, so as to give the whole a rudely conical shape, the base of which was already partly attached around its margin to the walls of the sac.

Methods of Procedure in Treatment by Galvano-Puncture.—The modes of performing galvano-puncture are numerous; and, concerning their respective merits or demerits very opposite opinions have been expressed by different physicians and surgeons. The subject itself is in its infancy; but the time at our disposal now will only allow me to allude to a few of the points concerning which a difference of opinion exists—and

this principally with the view of explaining the reasons for the course pursued in the present case, and of suggesting the principles of a safe and tentative mode of attacking sacculated aneurisms of the arch of the aorta or of other great arteries of the body.

The risks specially attendant upon this mode of treatment are thus referred to by Mr. Holmes in the admirable lectures to which I have already referred. He says (*loc. cit.*, p. 665):—"The dangers are principally from two causes—the inflammatory action produced in the sac and the cellular tissue which surrounds it, and the gangrene or ulceration of the skin at the points of entrance of the needles."

Whichever method we adopt, we must, therefore, strive to reduce these dangers to a minimum. The principal points to be considered in this relation have reference to the nature of the battery to be employed, the strength of the current, the mode and duration of its application, and the size and nature of the needle which it is most desirable to use.

As in this method of treatment we wish to make use of the electrolytic powers of the battery current and to avoid the heat-generating effect upon the electrodes, it is better not to use the more powerful combination and larger plates of a Bunsen's or Grove's battery. We may get all the good and none of the bad effects from the careful use of the battery which we employed in the present case, viz., Foveaux's modification of Smee's battery, made by Weiss and Co.

It is upon the question as to the most advantageous mode of employing the current that the greatest differences of opinion exist. Some writers recommend the insertion of the negative pole only into the sac, whilst the positive pole is applied to the integuments outside. Others recommend the introduction of two needles into the sac, one of which is to be connected with the positive pole and one with the negative, the current being allowed to pass all the time in the same direction, and in the hands of different operators, being either strong for a short time, weak for a long time, or even strong for a long time. But some of those who recommend the insertion of both poles into the sac, follow Pétrequin in advising that the direction of the current should be changed from time to time, during the same operation. And, lastly, a third set of writers recommend the method of procedure which we adopted in the present case, viz., the introduction of a needle or needles in connection with the positive pole into the sac itself, whilst the negative pole is applied by means of a wet sponge to the adjacent skin.

Now, before making any comments of my own on these different methods of procedure, I will quote the conclusions arrived at by Baumgarten and Wertheimer, as given by Meyer

(Electricity in its Relations to Practical Medicine, translation by Hammond, 1869, p. 470. He says:—"The numerous experiments made by them on animals invariably gave the following results. 1. If the needle connected with the negative pole was introduced alone into the blood-vessel, while the other needle was applied to the surrounding parts, no coagulation took place. 2. The introduction of both poles produced a slow, rather weak, and rarely perfect coagulation. 3. The introduction of the positive pole alone, with the application of the negative pole to the neighbouring parts, always brought about a rapid and complete coagulation." It would seem, therefore (if we are to rely upon the observations of the experimenters above mentioned), that the third mode of procedure is the best, even from the point of view of the readiness with which coagulation is brought about, and its satisfactory nature; but it was not for these reasons alone that I was led to adopt this method, and discard those which consisted of the introduction of the negative pole into the sac, either alone or in combination with the positive pole. Whilst these latter methods seem to me to be open to grave practical objections, I know no compensating advantages to recommend them. In several cases in which the negative pole only has been inserted into the sac, blood has actually spirted out or come away in smaller quantity on withdrawal of the needle (see Holmes's Lecture in Lancet, 1872, vol. ii., pp. 697, 708), and it has generally happened that bleeding has been more prone to occur through punctures which have been in connection with a negative needle. Then, again, the bulk of gas given off from the negative pole is always much greater than that which appears at the positive pole, and this leads to the formation, in the former situation, of a loose frothy coagulum, as any of you may easily satisfy yourselves by introducing the poles of the battery into a little egg-albumen. Now, independently of the *à priori* undesirability of liberating a larger quantity of gas than is necessary within the circulatory system, this liberation tends to produce effects which the case we have been considering to-day seems to show should be especially avoided. A copious liberation of gas is apt to cause swelling and distension of the aneurism, in confirmation of which I need only refer you to the valuable paper on "The Treatment of Aneurism by Electrolysis," by Dr. Duncan and Dr. Fraser (Edin. Medical Journal, August 1867). Now this of itself is a condition of things which we should generally strive to prevent. On the other hand, if we seek for the reasons which should induce us to employ the negative pole, none are to be found which do not seem to be born of an over-zealous wish to accomplish too much in a short space of time, whilst some of those who recommend it base their reasonings upon an

altogether unsafe foundation. They argue that adequate results cannot be produced with the positive pole alone and moderately weak currents, because, under totally different conditions, a wholly different fluid (such as egg-albumen) is not appreciably affected. But they should recollect that the changes in a fluid so chemically complex and unstable as blood, cannot safely be measured by those which will occur in a fluid like egg-albumen. And, moreover, if we be not dealing with a case where galvano-puncture is resorted to with the view of producing a considerable and immediate coagulation, with the view of warding off an actually impending rupture (as in the case recently operated upon in this hospital by Mr. Beck), I think we should always strive to advance slowly and cautiously. We should operate with the view of initiating the formation of a laminated clot by starting the formation of a small nucleus in connection with the wall of the vessel, rather than with the view of producing a large non-laminated clot immediately.

Because, therefore, the introduction of the negative pole into the sac can do nothing more, even according to its advocates, than increase the size and amount of the clot by a material which is admitted to be less durable and tenacious than that formed around the positive pole (Drs. Duncan and Fraser, *loc. cit.*, p, 14); because it is very questionable whether this is to be regarded as an advantage in the class of cases which we are now considering; and, finally, because its use seems to carry with it the positive disadvantages of favouring hemorrhage, of liberating an unnecessary amount of gas within the circulatory system, and of occasionally producing an unsafe distension of the sac itself, I am decidedly of opinion that it is undesirable to introduce both poles into the aneurism.

The method which I should adopt in any future case, therefore, would be that to which I have already had recourse; and you will find that in one of the most satisfactory cases yet on record of decided relief, practically amounting to cure, of an aortic aneurism, the treatment was conducted in this fashion by Dr. McCall Anderson of Glasgow. (Lancet, 1873, vol. i., p. 211.)

We come now to the question as to the nature and size of the needle to be used in galvano-puncture, and as to the strength of the current. The needles previously employed by most operators seem to have been larger than it is at all necessary to use, whilst some of them, such as Weiss's modification of Dr. Duncan's needle, are unquestionably very much too large. Recognising the desirability of reducing to a minimum the chance of hemorrhage through the punctures and of inflammation in the track of the needle, and, at the same time, of diminishing the painfulness of the operation, I was led to use a very small hare-lip needle. Of course, the needle must be

large enough, and used in such a manner as to avoid all chance of breakage. The smaller the needle, the greater would be the amount of heat developed in it by the passage of the current, if this were strong and evolved from a large-plated Bunsen's or Grove's battery; but, using such a comparatively weak current as we employed from one of Foveaux's batteries, we get rid of the heat-effects, and also, therefore, of any disadvantage which might otherwise attend the use of a small needle. But, whilst it is desirable to keep down the strength of the current as much as possible, in order to diminish the shock and pain which its passage may occasion (Dr. Duncan, *loc. cit.*, page 8), and also with the view of avoiding, as much as possible, the two special risks of inflammation in the sac, or of inflammation or gangrene of the skin at the seat of puncture, we should do all in our power to increase the amount of chemical action excited by the weak current at the positive pole. For you must understand that the experiments of Dr. Fraser and others have unmistakably established the fact that the coagulation induced in the blood by the passage through it of a galvanic current, is wholly attributable to the electrolytic or chemical changes which the current occasions. If, therefore, after the fashion originally introduced by Steinlein, we cover the positive pole with an easily oxidisable metal, such as zinc, we are enabled to summon to our aid what Faraday always distinguished as the "secondary" effects of electrolysis. The salts as well as the fluid of the blood are decomposed by the action of the current, and a part of the oxygen and acids which are liberated at the positive pole will combine with the zinc and corrode the surface of the needle, Some of the salts thus formed will aid in producing further coagulation around the needle. Corrosion occurred to a marked extent in our last operation, when a current from eleven cells of the battery was allowed to act upon a needle which had been plated with zinc, although no corrosion was produced in an ordinary steel needle submitted to the same influences. Of course, a gilt needle would be still less amenable to the action of these "secondary" effects of the current.

With regard to the strength of the current, it was found that eleven cells acting for half an hour, although producing no pain or obvious sensation at the time, did induce some amount of irritation of the skin around the punctures. The current from eight cells, however (the battery being in good condition at the time), produced, on the previous occasion, no irritation, so that all traces of this needle-puncture had disappeared in four or five days. This needle also was insulated within half an inch of its point merely by two layers of spirit varnish; and probably, in employing a comparatively weak current such as this, no insulating material is needed.

This, indeed, is the opinion of Dr. Althaus. At all events, it seems desirable that the needle should not be pushed too far into the sac, and that the part of the needle actually in the sac and within its coats should not be insulated, because clot should form in contact with the wall of the aneurism, so that it may be left adhering to it on the withdrawal of the needle.

Conducted upon the principles now laid down, the treatment of aneurisms of the arch of the aorta or of other large vessels may, doubtless, in a large number of cases, be safely attempted by galvano-puncture. The treatment should be painless, non-irritating, tentative, and slow; and I believe that it is a method from which, in the future, we may look to obtain the most satisfactory results.—*Brit. Med. Jour.*, *Nov.* 29, 1873, *p.* 623.

60.—CASE OF POPLITEAL ANEURISM SUCCESSFULLY TREATED BY CATGUT ANTISEPTIC LIGATURE.

By JOHN MORGAN, Esq., Professor of Descriptive Anatomy in the Royal College of Surgeons, and Surgeon to Mercers' Hospital, Dublin.

[The following case was treated in various ways, by position, by rest, and by different modes of compression, but without success. The case is peculiar, as being that of a woman. As it was going on from bad to worse and the pain was excruciating, an operation was absolutely necessary.]

I ligatured the artery in Scarpa's angle. The patient was put under the influence of ether, and there was no sickness or any other unpleasant symptoms. The incision having been made and the artery exposed, I passed an aneurism needle round the artery and secured it with a catgut ligature. I was about to pass a second ligature when a little vein got torn. I therefore did not pass it but closed the wound by four carbolic sutures. The woman was removed to bed, and from that time to the present there was not a drop of pus; the wound healed by first intention. I never saw the catgut ligature afterwards. The patient complained of none of that excruciating pain she had previously suffered; but curiously enough she complained of some pains shooting up the upper thigh on the outside and also in the leg. The tumour which before had become prominent became solidified. One spot for some time was a cause of uneasiness, but it gradually consolidated and the tumour became firm, and no pulsation was found in it from the time the ligature was applied. I saw her a fortnight ago and she was then able to walk about the room, and on comparing one limb with the other I found there was a difference of nearly three-quarters of an inch between the two.

The umour had almost entirely subsided, and the woman could walk about without any pain or uneasiness. I might mention that I gave her a medicine which I had found of considerable use, and which I am now giving to another patient—that is, sulpho-carbolate of soda. The case now under my care is that of a man whose leg has been smashed, so as to necessitate amputation. It is a most unfavourable case, the only thing in the man's favour being that he is but 23 years of age. I gave him the sulpho-carbolate of soda, and no untoward symptom has occurred. I have given it also to this woman and, I believe, with beneficial results. I have seen Mr. Tyrrell's case, which, I think, does not go on as favourably as the one just described; the tumour has not disappeared so quickly, but the result in both is equally satisfactory. Both resisted treatment by pressure and were cured by ligature.—*Medical Press and Circular, Jan.* 7, 1874, *p.* 3.

61.—CASE OF CIRSOID ANEURISM TREATED BY INJECTION OF PERCHLORIDE OF IRON; DEATH PROM EMBOLISM.

By JAMES F. WEST, Esq., Senior Surgeon to the Queen's Hospital, Birmingham.

Despite the brilliant success which has attended the employment of coagulating injections for the treatment of nævus in the hands of MM. Broca, Gosselin, Demarquay, Professor Pitha, and others, they are evidently dangerous remedies in certain cases, especially when the tumours are about the face and head; and it is incumbent on those who have had fatal cases to publish them as a warning to others not to use such injections in the future, but to adopt some less hazardous mode of treatment. Mr. Bryant mentions that he lost a patient from embolism a few minutes after injecting a nævus, though he does not say on what part of the body it was situated. Mr. Thomas Smith, in an excellent paper on the same subject, gives several cases in which a fatal result attended the use of perchloride-of-iron injections. Recently Mr. Kesteven has also called attention to an unsuccessful case in his practice; and although he attributes death in that instance to "spasm of the glottis, induced by mental emotion," I cannot help thinking the result was more likely due to embolism. The absence of a post-mortem examination, however, prevents the solution of the question.

Such experience as is afforded by these records and by the following case must make all thoughtful surgeons pause before recommending the injection plan of treatment, and induce them rather to have recourse either to enucleation, the galvano-

cautery, the ligature, or, what I very much prefer, the wire écraseur. In the Lancet of March 4th, 1871, I have reported several cases in which considerable nævoid growths were treated by me in this manner with success; and further experience has shown me that the écraseur is one of the best, if not *the* best means we possess for the removal of vascular tumours.

Alfred G. T., aged nine months, was admitted into the Queen's Hospital, under my care, on June 2nd, 1873, with a nævoid tumour, about the size of a small marble, on the right ala nasi, near the tip. It had existed from birth, but was slowly growing, and beginning to involve the left ala. It was soft and compressible, and over its surface large and distended capillaries were seen in three or four spots, each as large as a pea. Deformity from the prominent situation of the tumour necessitated operative interference, and as it was considered that no mode of operation offered so good an opportunity for the prevention of an ugly cicatrix as the injection of solution of perchloride of iron, it was decided to use it. At 11 a.m., chloroform having been given, three drops were injected by a hypodermic syringe into the lowest part of the tumour. The child struggled and cried, but there was no other unfavourable symptom. Three more drops were then injected into the upper part of the tumour, and three at another highly vascular spot, when, on withdrawing the syringe, the child's face changed suddenly to a dusky hue, the hands and feet became blue, the pulse could not be felt, and the breathing seemed to be arrested; on raising the eyelids, the eyeballs were seen to be fixed and turned upwards and outwards, and the pupils were slightly dilated. The tongue was at once drawn forwards, and artificial respiration kept up for twenty minutes. During some part of this time the child breathed naturally, and then would cease to breathe until Silvester's method was again used. Until 6 o'clock the same evening he continued in a half-unconscious state, with a very feeble pulse, dilated pupils, livid hands and feet, and the angle of the mouth on the right side somewhat drawn down. About every ten or fifteen minutes he would start crying, and then relapse into his former semi-comatose state. During the evening the extremities became warmer and of a more natural colour, the breathing less laboured, and the pulse better; but the eyeballs rolled about, and the child could not be roused to take notice, and could hardly be induced to imbibe milk from his bottle. He continued in this state for three days, when he died, with well-marked symptoms of hemiplegia.

At the post-mortem examination, the membranes of the brain were found slightly congested; the anterior portion of

both lobes of the cerebrum, and also the inner surface of the brain as seen in the walls of the lateral ventricles, were reduced to a soft pulp, while the posterior part of the brain-substance was in a healthy state. A clot was seen stretching from the right internal carotid artery along the middle cerebral artery of the same side. The lungs were congested. The heart contained a small quantity of dark fluid blood. The liver, kidneys, and other viscera were normal.

That embolism was produced in this instance by the injection of the perchloride of iron does not admit of a doubt, and I therefore unhesitatingly confirm Mr. Thomas Smith's opinion that we are justified in rejecting it as a remedy for nævi on the face.—*Lancet, March* 21, 1874, *p.* 402.

62.—THE WINGED SCREW TOURNIQUET.

By Surgeon Major A. MOFFITT, Netley.

In consequence of the reports of Professor Longmore and other writers on Military Surgery, regarding the evils arising from the constriction of the limb in the application of the tourniquet for the arrest of hemorrhage in gun-shot wounds, numerous experiments were conducted by me at Netley, with the view of obtaining an instrument, the use of which would not be attended with the serious and often fatal consequences described as occurring in warfare. The instruments at my disposal were, Lee's tourniquet, Mott's tourniquet, the field tourniquet of the British Service, Petit's tourniquet, Signoroni's clamp, and the artery compress used in the cure of aneurism.

The following were the qualities which it was decided a tourniquet for use in military surgery should possess: 1, portability; 2, simplicity; 3, good compressing power; 4 steadiness, so as not to shift position in lifting and laying the patient; and 5, not to constrict the limb.

Lee's tourniquet answered well for the arm, and was found to be portable, simple, and steady; but for the thigh it had not compressing power enough, and, in addition, it constricted the limb where the patient was at all muscular.

Mott's tourniquet answered fairly for the thigh, though deficient in compressing power; but in the arm, owing to its convex pad and peculiar shape, the slightest movement of the patient caused it to slip on one side or the other of the bone, where it could exercise no pressure on the vessel. It is not very portable, from its awkward shape.

The field tourniquet of the British service was found to have very little compressing power over the artery—the pad being too small. It, however, constricted the limb to a dangerous

extent, and often, when it had no effect upon the arterial circulation, it completely arrested a return of blood through the veins. It has no quality but portability to recommend it.

Petit's tourniquet was found to have a good compressing power, which could be easily and quickly brought to bear or slackened. It is also very portable; but it constricts the limb, is liable to shift its position, and is rather complicated.

Signoroni's clamp possessed power of compression and did not constrict the limb; but it is too clumsy for field purposes, and liable to slip off the vessel.

The artery-compress for the cure of aneurism was found to have good compressing power, and did not constrict the limb; but it is deficient in all other requisite qualities. The idea of depressing the pad by the screw is good, and is the principle carried out in the tourniquet which will presently be described.

Thus far none of the tourniquets tried came up to the requirements sought for. A combination of the good qualities of all was then attempted, and for this purpose Petit's tourniquet was selected, as the base on which to work. The idea of wings

Fig. 1.—The Tourniquet as applied.

and the shape of the pad were taken from Lee's. The wings, made of steel bars, were attached to the lower plate of Petit's tourniquet, and the strap carried across rollers at their points.

When open, these wings extended out at each side, so as to give a span of 6½ inches, which extends the diameter of any ordinary thigh. A quadrangular wooden pad half an inch thick, two inches long, and one and a half broad, and slightly concave on its under surface, was fastened by a loop on the under surface of the lower plate. The tourniquet thus constructed was tried and found to be portable, had good compressing power, did not constrict the limb, but was a little complicated, owing to the strap having to pass over so many rollers, and had a tendency to turn over the long axis of the limb. This tendency to turn over, however, was remedied by lowering the points of the wings; but then it was found these latter constricted the limb.

The wings were then attached to the upper plate, instead of to the lower one; the lower plate was removed altogether, and a small square plate substituted and attached to the pad by four screws, having the strap permanently fixed under it. The instrument thus constructed is shown in the accompanying figure (1). On trial, it was found to possess all the qualities sought for—it is portable, simple, has good compressing power, is steady, and does not constrict the limb. When folded up, it forms a package 3¾ inches long, 2 inches broad, and 1¾ deep (Fig. 2). There is a double shortening of the strap at each side, and in this way the requisite amount of pressure is rapidly

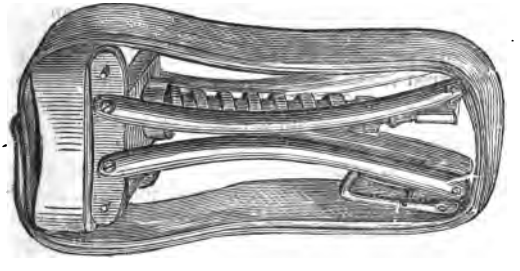


Fig. 2.—The Tourniquet folded up.

reached, the pad is pressed down upon the vessel by the action of the screw, as in the artery compress for the cure of aneurism, and at the same time the wings are raised and all constriction of the limb at each side of the pad removed. The double shortening of the strap is of great importance, as it was tried to cut the strap and sew the end of one piece round the cross bar at the point of each wing for the purpose of greater simplicity, and with this arrangement, which gave only a single shortening at each side, the screw reached the head before the requisite amount of pressure could be made.

As to the concave pad employed, instead of the usual convex one, I need only refer to Lee's arguments on this head.

It is a matter of importance that all old screw tourniquets are convertible into this pattern at a small cost, by the makers, Messrs. Evans and Wormull, Dowgate Hill, who ably carried out my ideas in the manufacture of the instrument.—*British Medical Journal, Jan.* 3, 1874, *p.* 16.

63.—ON THE SAFETY OF TORSION IN AMPUTATION.

By THOMAS BRYANT, Esq., Surgeon to Guy's Hospital.

The following case, brief notes of which I now forward, I think may be read with interest, supporting as it does the practice of torsion, and telling against the use of the carbolised catgut ligature.

On Friday, March 6th, I was called upon to remove a mutilated forearm from a man aged twenty. I amputated below the elbow-joint, and arrested bleeding by twisting all the arteries with the exception of the interosseous, to which I applied a ligature of carbolised catgut. I could not twist this artery on account of its intimate connexion with the interosseous membrane. At the end of six days hemorrhage took place from the stump, which was stopped by raising the limb; but as it recurred towards night the stump was opened. It was then made out that the bleeding had taken place from the interosseous artery. The vessel was again tied by my dresser, and the man has gone on well since.

This is the only case of secondary hemorrhage from a stump that has occurred to me since I began the practice of torsion of arteries in January, 1868; and it is interesting to know that in it the bleeding came from an artery that had been ligatured, and that the ligature was of carbolised catgut.

We have now had at Guy's Hospital upwards of 200 cases of amputation of the thigh, leg, arm, and forearm, in which all the arteries have been twisted (110 of these having been of the femoral artery), and no case of secondary hemorrhage.—*Lancet, March* 21, 1874, *p.* 417.

64.—A NEW ARTERY AND TORSION FORCEPS.

By T. SPENCER WELLS, Esq., Surgeon to the Samaritan Hospital.

Mr. Spencer Wells has introduced a kind of artery and torsion forceps, which very conveniently replace the old spring artery forceps of Liston, and the bull-dogs used for the temporary stoppage of bleeding vessels during operations, while

they are the most readily applied of any of the varieties of torsion forceps met with in the shops. They have scissors handles, and were first made for Mr. Wells by Krohne and Sesemann. The grasping and holding extremity is roughened by rather deeply cut transverse teeth, so that the bleeding vessel is forcibly compressed, and its coats squeezed or almost crushed together. This is alone often sufficient to stop the bleeding without any torsion, especially if the instrument be left on the vessel for a minute or more. But if the vessel be large, then two or more rotations may be added. Instead of the spring-catch, the fastening is effected by a Mathieu's catch in the handles. This is quite as easily fixed and opened as the spring, and is much less likely to get out of order. The instrument is made of steel, but is coated with nickel, which prevents any rusting after use. Mr. Spencer Wells, as is well known, trusts to the clamp for securing the pedicle in ovariotomy whenever it can be applied without too much pull on the uterus or broad ligament. Where the pedicle is very short or broad, he either uses the cautery, the ligature, or the *écraseur*. He has never trusted to torsion alone, though occasionally securing one or more vessels in this way. With the bleeding vessels in separated omentum, he trusts to torsion when the vessels are distinct; but when oozing surfaces rather than separate vessels are seen, he uses ligatures of fine pure silk, cutting off the ends short and returning them. He prefers this silk to catgut. With bleeding vessels in the abdominal wall, torsion, or simple compression with the torsion-forceps, is sufficient.—*British Medical Journal, Jan.* 10, 1874, *p.* 47.

65.—REPORTS FROM THE METROPOLITAN HOSPITALS ON THE USE OF TORSION IN SURGICAL OPERATIONS.

Torsion of arteries is a proceeding largely but very unequally adopted by modern surgeons. The procedure is commonly adopted at some of the metropolitan hospitals, but is rarely witnessed at others; in explanation of which fact it will be noticed (upon perusal of the following reports) that the opinions of the value of torsion entertained by different surgeons are widely divergent. Gentlemen who have most largely employed it are generally found to be those who most commend it; whilst dread of secondary hemorrhage, or other mishap, seems to deter many from even entering upon its use. Such theoretical fears, however, appear to be groundless, as the anticipated mishaps do not occur in actual practice. Mr. Callender, of St. Bartholomew's Hospital, remarked, in a lecture published in the British Medical Journal, on January 20th, 1872, "there is

no record, where the operation has been properly practised, of any sloughing of the twisted end, or of any abscess along the track of the vessel; and whilst the presence of a foreign body in the wound is avoided, the patient escapes the anxiety which the prospect of the removal of ligatures entails. And to add one other, and this a strong argument in favour of torsion, it is free from all risk of that secondary bleeding which is sometimes associated with the separation of a ligature." The opinion of Mr. Birkett of Guy's Hospital is equally strong in favour of torsion; he always uses it in his operations, and has never met with recurrent or secondary hemorrhage after its employment, nor with suppuration extending up the sheath of the twisted vessel. In fact, if the bleeding be once thoroughly arrested by torsion, it never recurs. Torsion requires considerable manipulative skill for its successful employment; a circumstance which young operators are apt to forget. Much time also is consumed where all the small arteries are twisted; consequently the catgut carbolised ligature is sometimes used for small vessels, and torsion is restricted to the main arteries—the very reverse, as Mr. Forster observes, of the precepts formerly inculcated. Want of space will not permit all the replies which have been received to appear now. Our thanks are due to the surgeons who have favoured us with the communication of their experience and opinions on the subject.

Sir JAMES PAGET writes: I have not any large experience of torsion of arteries. My impression is that we ought to learn which cases are best suited by each of the chief means of stopping hemorrhage—ligature, carbolised ligature, acupressure, torsion; and in each case to use the most appropriate means.

Mr. JOHN WOOD, of King's College Hospital, is of opinion that, although torsion is undoubtedly of value in arresting bleeding from the smaller arteries, especially in plastic operations on the surface of the body where extensive primary union is essential, yet its application to the larger arteries does not present so many advantages as that of the ligature. The grounds of his opinion are as follows.

1. Torsion is not so certainly effective against primary and intermediary hemorrhage as a well applied ligature. A surgeon obliged to leave his patient without immediate surgical supervision after an operation, cannot be so confident about his safety from bleeding as when ligatures have been employed.

2. The ligature is more speedy in its application in cases where many arteries are bleeding at once, and it is important to prevent much effusion of blood without other means of restraining it. However dexterously torsion may be applied, it occupies more time than the ligature.

3. In instances where the artery is cut off short in a hollow

part difficult of access, as between the bones of the leg in amputation below the knee, and when the vessel and the tissues in its immediate contiguity are diseased, thickened, and consolidated, the process of isolation necessary for torsion is tedious, and sometimes involves much loss of blood, and a good deal of violence to the diseased parts; while, on the other hand, the ligature, including perhaps a small portion of the parts adherent to the vessel, and thus supporting and strengthening it, is usually a quick and effective proceeding.

4. Arteries of unusual delicacy of texture, or softened and rendered friable by disease (constituting the most trying tests of any hæmostatic proceeding) are apt under torsion, especially unlimited torsion, to have so much injury inflicted upon their sheath-attachments and *vasa vasorum* for a considerable distance, as to become subject to disorganisation, suppurative inflammation, and burrowing of matter along their sheaths. In limited torsion, the pressure of the forceps applied transversely above the twisted part, necessarily inflicts a certain amount of damage upon the arterial coats at that point, rendering them more likely to slough or yield to the forces of the circulation, than when a ligature is applied, and such pressure is not needed. This is especially important in the not uncommon cases where an efficient internal clot is not formed.

5. Where a branch issues from a large artery close above the point of section, torsion involves the probability of such a branch either preventing sufficient and effective twisting, or becoming itself detached from the parent trunk, and thus forming a source of hemorrhage from the aperture of its exit, and perhaps also from its torn off distal end. Under the same circumstances, a ligature is usually easily and effectively applied.

6. In instances where torsion is hurriedly or roughly performed upon a large artery, the vein in its immediate neighbourhood, and often closely connected with it, is apt to be pulled about, dragged out of its sheath, or torn, in separating those connections. So the vein is rendered more prone to assume unhealthy inflammation, or pouches and interspaces are left, which may become the seat of putrefactive or suppurative processes in dangerous contiguity to the open end of the vein.

7. Neither limited nor unlimited torsion removes the possibility of necrosis of the twisted end of the artery, which would, in case of its occurrence, interfere with and prevent primary adhesion at that point, just as much as a ligature or any other foreign body, and leaves a less direct way for the escape of the slough, than a ligature does when left hanging out of the wound.

8. A metallic or hempen ligature placed upon a large artery, and left hanging out at the nearest and most dependent part of the wound, acts beneficially in conducting the discharges to the

surface from the dangerous vicinity of the large associated and contiguous vein, and this advantage more than compensates for the small interference with primary adhesion to which it may give rise. In very many cases also, these larger vessels lie in close proximity to a cut bone, which, from the shape in which its section is usually left, preventing close contact of the surrounding soft parts, is the least likely place in the whole wound for primary union to take place, and is the most usual seat of deep suppuration during the process of healing after amputation. In such cases, the ligatures may be made use of as valuable conductors of purulent discharges from a dangerous and often inaccessible neighbourhood. Mr. Wood usually utilises ligatures in this manner by passing carbolised drainage-tubes alongside or over them. When hempen ligatures are used by him, they are usually first well waxed and steeped in carbolised oil. In plastic operations, especially those involving the urinary organs, acupressure and silver wire sutures are employed.

9. Lastly, Mr. Wood is of opinion that, after amputation and excision, entire adhesive union is not so usual a result as to render it a matter of essential importance to avoid the presence of a few ligatures, which may interfere with it in their linear course towards the surface, but that, on the contrary, they have the more important advantage of acting as direct drains, providing against the not unfrequent deep retention of discharges which prolong the convalescence, and often seriously imperil the safety of the patient.

Mr. SAVORY (St. Bartholomew's Hospital) thinks torsion, which is an old practice recently revived, is one of the best means of arresting hemorrhage in certain cases; but, as a rule, for the arrest of hemorrhage from important vessels, he prefers ligature. He considers that we are not yet justified in assuming that torsion afford the same degree of security as ligature, and he believes that the objections to ligature when properly applied have been exaggerated.

Mr. COOPER FORSTER (Guy's Hospital) writes: The subject of torsion, so far as I am concerned, is pretty well used up, and I have nothing further to add to the subject. It is much more applicable to large than to small arteries, and easier to apply; this is quite contrary to what I was taught in my student days, when the twisting of large arteries, or of one larger than the radial, was never thought of. Mr. Morgan, however, taught us to twist a vessel of that size if we liked, though he never did it. The smaller vessels are troublesome sometimes to twist, and then the catgut ligature comes in very well, as it saves time. I never use any other plan than torsion for all my operations when hemorrhage has to be arrested, except as stated above.

Mr. THOMAS SMITH (St. Bartholomew's Hospital) writes: I have used torsion in surgical operations enough to know that in skilled hands it is an efficient and safe method of arresting hemorrhage from divided arteries, and that, in cases where a speedy union of the operation wound is desirable, torsion is superior to silk ligature, inasmuch as no foreign body is left in the wound. I rarely use torsion, on account of the time that is often consumed in securing the smaller vessels. I much prefer, and therefore employ, carbolised catgut, cutting off both ends of the ligature. This can be employed, without difficulty or loss of time, by an unskilled person, to all bleeding vessels; and it has the chief advantage that torsion possesses, namely, that no noxious foreign body is left in the operation wound.—*British Medical Journal, Dec.* 20, 1873, *p.* 723.

66.—EPISTAXIS SUCCESSFULLY TREATED IN TWO BAD CASES BY A SIMPLE PROCEDURE.

By Dr. WILLIAM WAUGH LEEPER, Loughgall, Ireland.

A few weeks ago I was in attendance upon a man in typhoid fever. The case pursued the usual course. On the fifteenth day there occurred profuse perspiration with abatement of all the grave symptoms; but on the night of the sixteenth day of the disease, some bleeding occurred from the nose. It was a mere trickle at first, but after the patient slept a second time the hemorrhage increased to such an extent that I was sent for. On my arrival, after a drive of four miles, I found the man cold and almost pulseless, with a constant effort at vomiting. The blood was slowly oozing from each nostril, and when he coughed or made an effort to speak, he spat up the same dark blood. The idea of plugging the nostrils occurred to me, but the operation seemed fraught with great danger, in the case of one in almost a dying state from exhaustion; so I thought of trying a remedy of easy application. I pushed up into each nostril two medicated bougies or suppositories of perchloride of iron. These I happened to have with me; they had been made (of double the usual strength) with two grains in each. I had them made by Duncan and Flockart, of Edinburgh, for another purpose altogether, *i.e.*, to push up into the uterus. I pushed two *up together*, and placed a bit of dry lint on the anterior nares. The effect was a present cessation of the bleeding, and I prescribed five grains of ergot, reduced to powder, every four hours. The result was successful for two days; when again in the middle of the night, the bleeding returned. The same remedy was again used, and with the

same happy result, and after that there was no return, and the man made a good and perfect recovery.

Case 2.—About ten days after this case I was called to visit an old man (aged seventy-two), suffering from epistaxis, which every remedy had failed to check. This hemorrhage also occurred in the middle of the night. When I reached the man's residence the bleeding had been going on for several hours. The old man was pale, face covered with cold sweat, and he had *frequently* vomited up blood. A stream of blood flowed from each nostril, and on looking into his fauces a similar stream seemed to flow down to the stomach, which accounted for the vomiting of such a quantity of blood.

I prescribed the ergot, this time the liquid extract of B. Ph. in scruple doses every four hours, and applied, precisely in the same way, the perchloride of iron bougies or suppositories. The result was the same as in the preceding case—viz., almost the immediate cessation of the hemorrhage, and in this case there was no return of it.

I have recorded these two cases simply because of the simplicity of the remedy used. These pencils soon melt, and the iron gradually comes in contact with the bleeding surface, and instantly its styptic effect is produced. The remedy is easy of application—much more so than plugging, and was in these cases more effectual than injecting the nares with the same remedy in a fluid form—a mode which I have occasionally tried. I attach great importance to the internal use of ergot in all hemorrhages, and hence I prescribed it in these cases. In hæmoptysis, and in certain hemorrhages from the uterus, I have found the sub-cutaneous injection of ext. erg., dissolved in glycerine, the most effectual and the promptest remedy.—*Dublin Journal of Medical Science, Nov.* 1873, *p.* 364.

67.—AN IMPROVED PLUG FOR EPISTAXIS.

By Dr. Henry Cooper Rose.

Having lately had a severe case of epistaxis, which was obliged to be restrained by the old-fashioned plug of lint drawn into the posterior nares by means of a string passed through the nostrils, my attention has been directed to the subject. It occurred to me that, if an elastic India-rubber bag could be introduced into the nose, and then either inflated with air or filled with iced water, all hemorrhage must inevitably be stopped.

I proceeded, therefore, to make an instrument in the following manner. I cut a No. 4 gum-elastic catheter into two equal lengths. At the end of one portion, I made small holes by

transfixing it with a needle heated to redness in a spirit-lamp. These holes extended, at intervals of a quarter of an inch, for the length of about two and a half inches. Over this punctured portion I tied firmly in two places, first at the tip, and then again about three and a half inches nearer the handle, a soft India-rubber bag, so as to include between the two tied portions all the perforated part of the catheter. To the open end of this catheter I fixed a short piece of India-rubber tubing furnished with a small stopcock.

Mode of Using.—All the air having been let out of the bag by opening the stopcock and gently compressing the India-rubber, somewhat after the manner of fastening an umbrella, the instrument is passed into the bleeding nostril. When it is coaxed so far that the end projects into the posterior nares, or as far as it can be, the nose of a small ball-syringe is applied to the mouth of the stopcock, and the bag is gently inflated while the air is being propelled into it with the right hand. The short piece of India-rubber tubing attached to the stopcock is compressed with the thumb and finger of the left hand at the right moment to prevent the air from passing out again; and then with the right hand the stopcock is turned. By this means, the inflated bag is made to fill all the inequalities of the nasal cavity, and a painless and perfect plug results. Should it be deemed advisable, iced water may be used instead of air. It may be found in practice necessary to have a thicker tissue for the bag, or even to use two, one over the other, to prevent bursting with the pressure employed, which, however, need not be great. Should it be found necessary to make the instrument stiffer while it is being passed through the nostril, the ordinary wire stilette may be introduced through the open stopcock. Each instrument should be accompanied by several supernumerary bags; and, before using the instrument, it should be dipped into warm water, to increase the elasticity of the India-rubber.

I believe that this contrivance possesses sundry advantages over the ingenious instrument devised by Mr. Godrich; *e.g.*, it is exceedingly inexpensive, quickly made and repaired, very easily introduced because of its small calibre, and the universal pressure over the whole nostril is much less annoying than when exerted over two points only.

I have taken a specimen of the instrument to Messrs Coxeter, of Grafton Street, East, who will have some prepared for inspection and trial. It is well adapted to hospital practice, because the introduction of it for an hour will probably stop hemorrhage; and the removal is effected very easily by simply turning the tap and letting out the air.—*Brit. Med. Journal*, Jan. 10, 1874, *p.* 49.

68.—THE EMPLOYMENT OF THE CAOUTCHOUC ELASTIC LIGATURE.

By Dr. FRANCIS J. B. QUINLAN, Physician to St. Vincent's Hospital, Dublin.

After some introductory remarks upon the different modes of removing vascular growths by the galvano-cautery, the écraseur, and by ordinary ligature, Dr. Quinlan said :—

The case under our consideration, and lately under your observation, is that of Bridget G., a healthy, well-nourished countrywoman, of plethoric temperament, aged twenty-seven years. Upon examining her, we found, at the upper part of the vulval opening, a large vascular tumour, of the size of a very large orange. This tumour was of several years' standing, and bore an exact resemblance to a small cauliflower. It was connected with the site of the clitoris by a pedicle resembling the stalk of that vegetable, which was topped with the arborescence of the tumour. From either side of the pedicle descended two hypertrophied labia minora, like enormous cocks-combs. The case was evidently one of syphilitic hypertrophy —a diagnosis which was thoroughly confirmed by crops of condylomata around the anus and on the folds of the nates. The cauliflower tumour, which was divided into two portions by a fissure extending to the very root of the pedicle, was evidently the hypertrophied prepuce and frenum of the clitoris, and the peculiar arborescent appearance was attributable to the trabeculæ of the erectile tissue in this part of the labia restraining the hypertrophy of the rest of the tissue, and, by binding it down in bundles, producing the arborescence. The condylomata and the local irritation being reduced by rest and appropriate treatment, the question arose as to the best and safest method of removing this disfiguring hypertrophied mass; and it struck me that the newly introduced elastic ligature would admirably act in the case of this most vascular growth, so liable to cause dangerous hemorrhage. Accordingly, on the 20th of December I ligatured the whole mass with a solid india-rubber cord two millimetres in thickness. I used this solid cord because the result of some previous experiments had satisfied me of the inadequacy of drainage-tube or any other variety of hollow caoutchouc tubing. The solid cord which I used is to be obtained at most india-rubber shops, the only difficulty being to procure it of sufficient thinness ; when thicker than I have mentioned, it is unmanageable, especially for small growths. When drawn out to four times its rest length, this cord is not more than half a millimetre in thickness, and the tension upon half a metre of it is 1230 grammes. It is thus admirably adapted for this gradual ligaturing and cutting process.

Having etherised the patient, I tightly ligatured the whole mass, tying the knot by the ingenious plan, devised by Sir Henry Thompson, of the addition of a series of ordinary ligature knots of sewing silk to prevent the india-rubber knot slipping. Immediately the temperature of the tumour fell from 98·3° F. to 96·5°. After a few hours it rose again slightly, and the tumour became very tense. The temperature then fell again rapidly, and the sphacelating process steadily continued until the growth became destroyed.

The pathological process thus occurring is plain, and it is in this that the great merit of the elastic ligature consists. When it is applied there is a brief interruption of all circulation, but quickly the arterial flow recovers itself, the venous system being still impeded. Great distension of all the capillaries naturally ensues, superinducing a condition of things resembling that which is the cause of varicose ulceration of the legs. The danger of the process (I speak solely from theoretical considerations) appears to be the presence of a mass of gangrenous organic matter immediately opposite a granulating and highly absorbent surface. For this reason in our recent case we kept outside the india-rubber ligature a loose skein of cotton yarn, about the same thickness as the ligature, and well soaked in carbolic oil. This was renewed three times a day, and a piece of lint kept well wetted with a permanganate solution prevented any offensive odour. On the third day it became evident that the ligature was no longer acting, and on examination it was found to be quite loose. You may remember the simple way in which this difficulty was combated. A slender rod of ivory, one millimetre in thickness, was passed through the ligature circle and twisted round a few times, in the manner in which a ready tourniquet is made from a handkerchief tied round a limb with a stick twisted through it. These ivory rods can be had of various thicknesses in the shanks of ivory crochet-needles. Ivory is, I think, the best material, on account of its strength, lightness, smoothness, and rigidity; and by this simple means ligatural pressure can be kept up to the very last to any degree of tightness required. On the fifth day the mass dropped off, leaving a surface which healed up kindly; and, with some constitutional treatment for the syphilitic taint, the patient made a good recovery.

I would particularly impress upon you that at the time of application, and while it is cutting through the skin, the ligature is painful. For this reason, just before commencing, we injected hypodermically a quarter of a grain of acetate of morphia in the neighbourhood of the operation, a procedure which much assisted the anæsthesia, and reduced subsequent

local pain, by keeping the patient throughout the whole day of the operation in a drowsy, quiescent state.—*Lancet, March* 7, 1874, *p.* 331.

69.—ERECTILE TUMOURS TREATED BY CROTON OIL.

According to Le Progrès Méd., 28th November, several physicians, among others M. Lafargue, have employed croton-oil in the treatment of erectile tumours, and we have already referred to a case by Dr. de Smet reported in the Presse Méd. Belge. In response to the inquiries of several correspondents we add the following particulars of Dr. de Smet's interesting case:—A patient, aged five, presented below the right lower eyelid an erectile stain of little extent surrounded by a zone the size of from 15 to 58 millimètres, in which were numerous small superficial vessels. The stain and the zone united are rather larger than a piece of 50 centimes. The centre is slightly salient. After some fruitless attempts (vaccination and repeated instillation of perchloride of iron) Dr de Smet had recourse to croton-oil, and he thus describes the method used: Some fifteen needles were fixed in a piece of cork and the points were free for about 2 millimètres. These points were thus disposed so as to represent as well as possible the shape of the spot and the direction of its principal vessels. This being done the cork was dipped in croton-oil and then accurately applied over the spot and quickly pressed upon it. The inoculation, painful for the moment, only left after it a slight sensation of heat and pricking. A little cotton-wool was used for dressing. Next day the parts were swollen, and there was a slight vesication, but no pain. On the second day crust surrounded with little vesicles and a certain number of the vessels had disappeared, others were stopped up by clots. Another inoculation of croton-oil. In four days there was no trace of the nævus, nor any scar left.—*Medical Press and Circular, Dec.* 10, 1873, *p.* 530.

70.—A CASE OF SUDDEN DEATH QUICKLY FOLLOWING THE INJECTION OF PERCHLORIDE OF IRON INTO A NÆVUS.

By W. B. Kesteven, Esq., London.

Fatalities are often more instructive than successes in surgery. They point to sources of danger to be avoided, and compel us to cast about for conditions of safety. For these reasons I have thought that it might be advisable to add the following to the list of casualties that have been recorded in connexion with the treatment of nævi by the injection of perchloride of iron.

On April 3rd, 1873, I injected with perchloride of iron a nævus on the head of an infant aged nine months. The nævus was of a circular form, was situated on the top of the head, over the upper border and middle line of the frontal bone, and was about three-quarters of an inch in diameter. No ill effects followed, and the operation was apparently successful. In about three months afterwards, however, a re-appearance of the growth began to show itself, steadily increasing in extent, so that six months after the first injection it was determined to repeat the operation. By this time the fontanelle was closed, and the child in average health, save that it was excitable, and subject to child-crowing. On Oct. 4th five minims of perchloride were taken up in a graduated syringe with a screw piston, and, my son assisting me, three minims were injected; the rest escaped from the wound. The child cried a good deal during the few seconds occupied by the operation. In a short interval of time, it may have been a minute, it again began to cry, then suddenly turned pale and was slightly convulsed, at the same time that it began a series of eight or nine shrill laryngismal cries, attended with distinct struggles to recover its breath, which suddenly ceased in death. The whole time that elapsed from the first insertion of the needle to the child's death could not have exceeded five minutes.

In the Lancet for August 17th, 1867, Mr. Thomas Smith of St. Bartholomew's, has collected several fatal instances following shortly upon the injection of nævi with perchloride of iron. In these cases the nævi were situated upon some part of the face or near the veins in the neck. As the result of these fatal consequences, Mr. Smith concludes:—"Sufficient is known of the effect of the possible admixture of perchloride of iron with the general circulation, from injecting nævi on the face, to justify us in rejecting it as a remedy for nævi in these parts, unless, by pressure or by the employment of some instrument, the circulation in the growth is controlled, at least for some time."

In the above-mentioned case, the child, as already stated, had been the subject of laryngismus, in a paroxysm of which, doubtless, it died. I had no opportunities of ascertaining by post-mortem examination whether coagulation of the blood in any veins had occurred, but since no accident followed the former injection, and as the nævus was far away from the veins of the face and neck, I am opinion that death in this instance was not the result of embolism, but took place from spasm of the glottis, induced by mental emotion. A fatal result would, I believe, have followed had any other mode of operation been employed.—*Lancet, Feb.* 7, 1874, *p.* 195.

71.—ON HARE-LIP.

By Sir WILLIAM FERGUSSON, Bart., F.R.S., Surgeon to King's College Hospital.

[Prior to Sir William Fergusson's investigation of this subject but little was known as to the anatomy of cleft palate. Few men could state positively, or could show by specimens, what was the nature of the substance of the intermediate hard material in the midst of the two gaps in the upper alveolar ridge, in a case of double hare-lip.]

The development of the teeth in these cases has, in my opinion, attracted less attention than in the normal condition of the upper jaw; but it is less my intention to refer to such anatomical and physiological matters than to some points in surgery, which I think specially worthy of notice as part of the result of my long experience in such cases.

First, as to the date or age when operations for hare-lip are most eligible, there is no doubt in my mind that an early period is now mostly preferred. The custom of delaying until after the completion of first dentition has now almost passed into abeyance, and the once prevalent idea that infants were specially liable to fatal convulsions after such operations may be said to have faded away. Experience has shown that they may be performed with safety at any period from a few hours after birth. Even at that dreaded period, first teething, harm is no more likely to happen than at any other time, provided always that the child be in good health. My own favourite date is from three weeks to three months after birth. Healthy-looking children at birth often pine and languish within the first few weeks, especially hare-lip cases, and particularly if the palate be implicated. I think it best to wait, in such instances, to see that health flourishes. A few weeks will give the requisite proof, and then, for many good reasons, I am of opinion that an operation should not be further delayed. I have a strong impression, from my experience, that the older a patient is, the more is the effect of the operation perceptible on the constitution, bodily and mentally; and I am equally convinced that, if it be performed at an age when there can be no subsequent recollection of the circumstances, so much the better for all parties. The parents and others interested in the infant are almost invariably in an unhappy state of mind until the operation is satisfactorily over. Maintaining, as I do, that the younger the patient is after the first three weeks, the safer and better is the operation, I consider it disloyalty to surgery to delay such a

decidedly advantageous proceeding on doctrines which will not bear the smallest scrutiny.

In the case of double hare-lip, with double cleft in the alveolar ridge, there may be great projection, or little or none. In the latter case, particularly if the columna and lateral portions of the lip be of good size, there may be no need for meddling with the intermaxillary mass. If, however, the projection be considerable, or what may be called great, and if the columna and side portions of lip be scanty, there ought then, in my opinion, to be no hesitation about taking away the projection at its junction with the vomer. The attempt to push this part back by gradual pressure is troublesome, or well-nigh impossible, in most instances, even if, as has been proposed, its narrow neck be broken. In either instance, it has never been told from experience in what condition, in what direction, the teeth come in the part thus displaced. In either or both instances, I have no doubt in my own mind that the teeth, if they came at all, would so slope backwards as to be of no value either for show or for use. There is, however, indubitable proof that, without thus meddling with the part, there are only two incisors of respectable size, after all, and these are of such indifferent quality, that they had better have been dispensed with at the earliest date. I, therefore, never hesitate to remove the intermaxillary mass when it seems the least in the way of a satisfactory operation. The advantages of doing so seem to me greatly to preponderate, and, if there be cleft hard palate at the same time, there is far greater chance, in after years, of the gap becoming narrower, whilst, in adult life, there will be greater facility for the assistance of the dentist. But, I imagine, there is less hesitation or difficulty in the surgeon's mind in the case of capacious double gap, than when there is only a single one with considerable projection of the intermaxillary margin. It is to such cases that the chief object of these observations is directed. If it be difficult to apply compression on the intermaxillary portion in double cleft, it is still more so when only one side projects; for its base is broader and firmer. The instances where there is no special projection are common, and require no comment, as there is then, as regards this matter, no obstacle to a satisfactory and successful operation; but, when there is a projection, if considerable, it is a more serious obstacle to these results than those inexperienced may imagine. I believe that this condition is a frequent cause of failure in the ordinary operation, particularly if it be done without the truss-compressor on each cheek to push the lateral portions of the lip towards the mesial line. In such a case, the surgeon is naturally anxious to leave the alveolar ridge untouched, and, in accordance with a common practice, when it is desirable to

secure union by first intention, when the stitches or needles are withdrawn, strips of plaster are carried from cheek to cheek to hold the union firm. Scarcely a greater mistake can be made, for, the line of union in the lip being generally, under such circumstances, exactly over, or opposite to, the sharp angle of the projection of bone, the young cicatrix is pressed against it, and gradually thins away, until it is fairly split open, when the operation proves a failure. This, I have a strong impression, is an explanation of the failure of many cases that do not seem, in any special way, complicated. I do not mean that straps always conduce to this effect, and that, therefore, they should never be used. On the contrary, I have very frequently seen them of much service. But, if the single projection alluded to be conspicuous—in which case, there will always be a somewhat sharp, angular margin—it is, in my opinion, best to get rid of it at the time of the operation. In my own practice, I was at one time in the habit of cutting the projection away with sharp small bone-forceps, dividing gum and bone at the same time, and aiming chiefly at getting rid of the projection. This usually involved all the intermaxillary bone on that side, and implied, perhaps, little heed of what damage might be inflicted on the sound side, although, latterly, I always passed the blades into the mesial line between the intermaxillary bones, so as to secure this side from material injury. In the course of my experience, I fancy that I refined on this practice. I found that it was well to detach the portion as high up towards the nostril as could conveniently be reached, and here I discovered that, in all young subjects, there was only cartilage to be divided. This could easily be done with the knife or scissors, and so, for many years, I have used only one or other of these instruments. Usually, I have passed the scalpel through the mucous membrane, under the frænum, up between the bones, and divided the cartilage, periosteum, and gum, to sever the part; and thus the use of cutting bone-forceps has been dispensed with, for, to say the least, such an instrument is coarse-like and clumsy in an operation for hare-lip on an infant only a few weeks old.

Whilst I can offer little objection to this proceeding, I fancy that I have recently fallen on one equally efficacious and void of certain objections which, I think, might be urged against it. Instead of this sweeping wholesale abstraction, I content myself with making an incision, vertical, sloping, or horizontal, with a scalpel through the mucous membrane and periosteum, over the projecting piece of bone; with a few touches of the knife, or a little squeeze with finger and thumb, I so separate these tissues as to permit the entrance of a gouge of a quarter or three-eighths of an inch in breadth, with which I scoop out the body of the milk incisor-tooth in as far as it is formed, taking

no heed of the cyst or of that of the permanent one, and even cut out such wall of bone as may be there; usually, at four or eight weeks, only small plates of bone. In this way, the hard projection is removed, and the tissues that remain offer no obstruction to the union of the junction of the lip in front, whilst the operation, as it appears to me, is less destructive, therefore more conservative, in character. There is thus left only the mucous membrane, with possibly some periosteum, which form a soft cushion behind the wound in the lip, and so the remaining intermaxillary bone is not divested of covering so thoroughly as when cutting instruments are passed in the mesial line to take all away on the offending side.

I have now adopted this plan in several cases, and have been much pleased with the effect and result. In one instance, I used a silk stitch to hold the edges of the wound in the mucous membrane together, but I doubt if it be needful, and I have not seen any spurt of blood from the deep part of the wound, such as that which I have often found, in other instances, has required the application of a pointed heated cautery. The wound has healed without attracting special notice. In the case where the stitch was used, the thread came away spontaneously, and, in the end, the gum appeared as if there had been no projection, and, therefore, no such operation.—*Brit. Med. Journal, March* 28, 1874, *p.* 403.

72.—ON CLEFT PALATE.

By Sir WILLIAM FERGUSSON, Bart., F.R.S., Surgeon to King's College Hospital.

It is now more than thirty years since I proposed the application of the practice of myotomy in the operation for cleft palate. Excepting in a few instances, objection has never been taken to the theory. There are instances where division of the levator palati muscles need not be resorted to, and union may with confidence be anticipated, but these are the exceptions; and doubtless it is owing to this preliminary step, that I can boast of a success well nigh equivalent to the usual result in hare-lip operations. Half-a-dozen instances of non-union are all that I can number in between two hundred and three hundred operations. The result contrasts favourably with that of Roux, who, as is recorded, was well content with success in two out of every three cases on which he operated—amounting to about one hundred and twenty operations in all. My theory, I maintain, is as sound as that in the operation for strabismus; yet I congratulate those who have succeeded without its application, and I freely admit that there are instances where the

operation may, and does, succeed without this preliminary to the ordinary process.

I claim to be the first in this country to have drawn special attention to Dr. Mason Warren's method of dealing with fissure in the hard palate. The proposal of that surgeon to peel the soft tissue off the bone on each side of the fissure in the hard palate has been extensively acted upon with varied modifications, but with very indifferent success. For myself, I may say that I have tried perhaps every plan that has been suggested, but I have been sorely mortified with the results. Some cases, after one or more operations, have turned out admirably; but in a large number the result has been such as to have made me despair of closing these openings by operative means. There are instances where such hopes should never be entertained, owing to the wideness of the gap and configuration of the parts; but there are probably as many as one half of the cases amenable to operation which have such slight deficiencies in the hard palate, that I have always acted on the idea that closure in the mesial line would or might be effected here as in the soft palate. Experience led me to give up the plan of Mason Warren of trying to close the gap in both soft and hard parts at the same time, even in instances where the defect in bone was but slight. For many years latterly I have ceased to meddle with the front part of the gap where the bones have been implicated, reserving the attempt for a subsequent operation; but, to facilitate the approximation of the margins of the soft palate, I have often followed the plan practised by Roux, of making transverse incisions, so as to divide the soft parts from the posterior margin of the osseous palate. Whilst I have had reason to feel satisfied with this step, I have in no way found my efforts to close the hole in the hard vault more successful. From time to time, on subsequent operations for the express purpose of closing such openings, I have been highly satisfied with Mason Warren's plan of separation—that of paring the soft tissue from the hard, by working from the margins outwards to the alveoli, sometimes, in addition, making the lateral incisions proposed by Mr. Field of Brighton, or by making these lateral incisions first, and separating the soft tissues towards the mesial line. In some instances, I have had good results from a sort of gliding process, facilitated by separating the front end of one of the flaps from its connections; but, altogether, the success of these operations has not been equal to my anticipations or desires. Since using chloroform, I have been solicited by patients, again and again, to try further operations, but the results have led me to give little encouragement to such proceedings. Latterly, however, a dawn has opened on my hazy views in this respect; and my chief object in

putting these observations on cleft palate together is to draw attention to my latest experience on the subject.

My impression is, that the frequent failure in this latter kind of operation is induced by the contraction of granulations, whereby the lateral portions are so drawn towards their original position, that the union in the central line is either prevented or broken, and thus the gap remains when the side-flaps have resumed their original positions. An idea came into my mind years ago which, however, I thought so wild, that I could not dare to bring it into practice. Repeated failures, however, by usual methods, brought it more forcibly into my thoughts, and at last I resolved to put it into execution. My project was that, instead of making the separation between the soft tissue and bony palate, for a quarter of an inch or so, I should divide the palate, soft tissue, and bone, about a quarter of an inch from the margin of the gap on each side, cutting the soft tissue in the roof of the mouth with a scalpel, and the bone, with mucous membrane above in nostrils, with a chisel, by means of which I could push the margins towards the mesial line; so that, having been previously made raw by removing the mucous membrane, they might be brought into apposition and held so by stitches.

This project was carried into execution in King's College Hospital on November 22nd, 1873, in the following manner.

J. H., aged 18, had the soft palate closed two years ago; a small aperture remained in the hard, which had been twice operated on unsuccessfully by the ordinary proceeding. The patient was placed under the influence of chloroform, and the mouth held open by Wood's gag. The edges were made bare by dissecting off the mucous membrane; then, by means of a small scalpel, an incision was made on each side. The back part of these wounds penetrated the soft palate, and in front they were close to the bone. Then the point of a chisel was forcibly, but carefully, pushed upwards through the bone into the nostril through each wound, and, by slight lateral movements of the blade, each lateral portion could be readily made to meet the other in the mesial line, whereby the raw margins made at first could be placed in apposition. The parts were then held together by a single stitch introduced in the usual way, passing, on each side, through the soft tissue, that it might remain steadily in one place. At the conclusion, the conditions seemed much as after the operations formerly effected. Subsequently, granulations filled the lateral gaps as on previous occasions; but, when the stitch was removed, union in the centre seemed firm, and was not disturbed by any dragging power, such as I supposed had, in earlier operations, drawn the flaps upwards and outwards towards the bone. The result was perfect, and I

have heard nothing to the contrary since the patient left the hospital. On the same day, I repeated similar proceedings on a patient who had had a successful operation on the soft palate, but on whom I had been unable, after four different attempts to close a fissure in the hard. Here the opening was larger. Similar steps were followed, and, to all appearance, during the first eight days, with similar results. Two stitches were employed, but, on removal, the edges, where one had been, seemed to fall asunder. Two days afterwards, a new stitch was put in, by means of an ordinary aneurism-needle, and the margins made raw by a gentle scrape. The hole was thus closed; and, on removal of the stitch, eight or ten days later, union was perfect. Since this patient left the house, a small separation has taken place, which has left an opening a little larger than the point of a probe. I have recently had made, by Matthews Brothers, a modification of a most ingenious needle suggested long ago by my friend Mr Brooke, of Westminster Hospital, for introducing stitches into the soft palate in operations on cleft in that part. The point of his needle was sharp; and I have often used it with much satisfaction. The point is blunt, like that of a common aneurism-needle.

Soon afterwards, a third case came under notice in private practice, wherein, after closing the soft palate, I had failed to succeed with the hard, by the ordinary proceeding. Here the opening was midway in size between the cases above related. The steps were much the same as in the first, excepting that I passed the threads through the lateral apertures without fixing it in the soft tissue. After the knots were tied, they never slipped from their position. These were left some days longer *in situ* than in the previous cases, and on removal, union in the mesial line was perfect. This union, I have recently heard, remains perfect.

A fourth case has recently come under my care at King's College Hospital. The gap here was larger than in the three preceding cases. It was an inch long, by more than a quarter of an inch wide. The lateral incisions were of proportionate length, and when the parts were brought together, a gap into each nostril was visible. Here three stitches were used, and, as in the case last related, they were passed through the side incisions, without piercing the soft tissue.

A fifth case is now under notice. Here the ordinary operation was performed on the soft palate in June, 1873; and the front part, which involved a small portion of the hard palate, was left untouched, in the conviction that it would be better to leave it alone until a future date. The aperture left was at the end of the healing process, of this size. Here the scalpel and and chisel were used as above described, and the parts were satisfactorily placed for union in the mesial line.

From the experience already obtained, as also from careful consideration of the subject, I feel justified in calling attention to this addition to what is already more or less familiarly known to those who have studied this complicated subject. At first thought, several strong seeming objections arise. It might be doubted if the osseous palate could be cut and moved in the way described. If an experiment were made on a roof of bone in the natural condition, it would be impossible; for, if the chisel were pushed through the bone a little on each side of the mesial line, it would be impossible to move the intervening portion towards the middle line, because the space is already filled up. This objection does not hold, however, in the malformation; for the space is not filled up, and the vacancy permits the osseous margins to be approximated. Then, division of bone on the two sides, and breaking the front part of each, seems so rude, rough, and destructive that the idea arises that caries or necrosis might ensue. But in reality the process is, in my opinion, less hazardous than when the flaps of soft parts are dissected or forcibly pushed or drawn off the bone. Besides the fact that these parts do not always unite, one flap or both will occasionally slough, and so things are rendered worse than ever.

In favour of the proceeding about which I now write, I can state from experience that the loss of blood is much less; and on that account there is less trouble in mopping out the pharynx, and consequently less hazard in the use of chloroform; that the tissues on the lower surface of the bones are not so much disturbed or divided as by the older process; that the periosteum and their tissues below are less disturbed than otherwise; and that, from all these circumstances, there seems less risk of sloughing of the semi-detached parts.

As to the healing of the lateral wounds, I am of opinion that the gap will invariably be closed, and that in the bond of union there will be a firm cicatrix of soft tissue, and possibly bone; that the vault of the mouth will be as firm as in a normal condition, and probably firmer than where a fortunate result has followed the process of Warren, Field, or Langenbeck.

These observations apply solely to instances where the surgeon, in a first operation, has not attempted to close the gap, or has failed in a design to secure union throughout. They will apply, however, with equal, even greater, effect to instances where heretofore, in my own experience, I have left the front part of the cleft untouched. I remember scores of instances of the kind, wherein I now feel confident the whole gap could have been closed by the process above described, with, as I imagine, as much success as attended the operation on the soft palate. In cases of defective hard palate, after the

usual preliminaries, and having bared the edges of the gap in the front, in hard as well as in soft, I would now make the additional wounds with scalpel and chisel, as above recommended, thereby hoping to avoid a second operation by doing all at once, under the beneficial influence of chloroform.

Since the above was written, I have had an opportunity of putting these latter views into execution. A youth of eleven years of age, on whom I had operated soon after birth for wide hare-lip, was brought under my notice, with a fissure extending from the lip to the uvula, A year ago, I had advised delay of a proposed operation on the gap in the soft palate, which was very wide. Now, I thought I should venture to test the proposal in an unusually severe case. Under chloroform, the usual incisions for dividing the levator palati on each side were first made; then the edges of the gap were pared as far forwards as to within half an inch of the front; next the scalpel and chisel were used in the way described; and finally, six stitches were introduced—two through the hard part, without piercing the tissue; and four through the soft parts, in the usual way.

Unfortunately, the first two stitches slid together, so that practically they acted only as a single one. The effect was, however, highly satisfactory. There was no tension in the front of the gap in the soft palate. The approximation of the bones took off all drag, such as is common under ordinary circumstances; and the result has been equal to my anticipations. The whole of the gap in the soft palate has united, and even a considerable part in the hard. With another operation in front—a repetition of this new process—I am convinced that the gap may be closed up to the alveolar ridge. I have no doubt whatever that, in instances where there is only a slight fissure in the hard palate, the surgeon, by use of the chisel, will thus be enabled to close it, and that in the soft with a certainty hitherto very questionable.

In my anatomical knowledge of such malformations, there is one condition which I fancy might prevent these views being carried into effect. In the generality of severe cleft in the hard roof, the lower and back part of the vomer is incomplete; but in some cases the vomer is entire, although perhaps swayed to one side; and it remains attached by its lower margin to one side of the hard palate throughout. Here would be a difficulty which might puzzle; for, although the chisel might permit the approximation of the edges, it would be difficult to introduce stitches; but even here there would be a redeeming quality in the new process, for pushing the parts to the mid-line might lay them so together that,

though union should not occur, they would lie so close that a fissure might elude ordinary observation. In one of the cases above recorded, I have found that entire union in the mid-line has really not taken place. The margins are, however, in such close approximation, that the want of union can be ascertained only by slipping the point of a probe through the suspicious-looking part.

With these remarks, I leave the subject for the present, under conviction that I have touched new ground in a most complicated field of anatomy and surgery, and in sincere hope that I have added to, if not completed, the power of surgery in such cases of malformation.—*British Medical Journal, April* 4, 1874, *p.* 437.

73.—UMBILICAL HERNIA IN INFANTS.

By JOHN GORHAM, Esq.

An experience of forty years of active practice enables me to speak very decidedly as to a plan of treatment which has been adopted, and in every case with uniform success, so that I am not able to point to a single instance in which it has failed. I am induced therefore to request an insertion of the plan adopted in the pages of the Medical Times and Gazette. It is as follows:—Having placed the child on its back in the mother's lap, the hernia is pushed in with the forefinger. The skin of the abdomen on either side is now pinched and folded up with the finger and thumb, and brought over so as to make both folds meet. The hernia now lies underneath the folds, which are represented by two parallel lines in contact with one another. Holding the folds in close apposition, by applying the thumb and forefinger of both hands above and below, the mother or an assistant now applies six strips of diachylon plaster—four transverse, two oblique. The transverse are three inches long by half an inch wide; the oblique are four inches long by half an inch wide. The small belly-band which has been used from time immemorial is now applied, and serves to keep the plasters in their place. These plasters will remain on, although the infant is washed well every night and morning, for four days, when they can be removed, and fresh ones substituted. It may be as well to notice, however, that the whole of the strips should not be removed at once, but one by one; and as fast as one strip is removed another fresh one should replace it. In this way the chances of protrusion during the crying or coughing of the child are much diminished.—*Medical Times and Gazette, Dec.* 6, 1873, *p.* 648.

74.—IMPROVED ENEMA AND DOUCHE, WITH BIDET.

This apparatus is made of strong tin, mounted on a stand, and enamelled white inside; the outside being enamelled in imitation of wood, marble, or china, as desired. The back part forms the bidet, upon which the person using it is seated; the back forms a compartment for holding warm or cold lavements or injections, and in which the pump is fixed. There is also a silvered jar for medicated lotions or enemata. Both pump and jar can be instantly removed for cleaning, by lifting out the small band round the pump, by which it is held firmly in its place. The tubes from the pump are jointed in such a manner that they can be raised or lowered at pleasure; and each of the injection tubes can be reversed from the end to the middle joint, or *vice versâ*, so as to serve every form of application, for male or female use. It has an elegant appearance placed in any bed- or bath-room, and is most simple in construction. It is manufactured by J. & F. Allen, 64 and 65, Marylebone Lane, London, W.—*Lancet, Feb.* 14, 1874, *p.* 235.

ORGANS OF URINE AND GENERATION.

75.—ON THE FUTURE OF OPERATIVE SURGERY FOR STONE IN THE BLADDER.

By Sir Henry Thompson, Surgeon Extraordinary to the King of the Belgians, and Professor of Surgery at University College Hospital.

I commence then, boldly, by saying that I have come to the conclusion that stone in the bladder, like many other maladies, is an exterminable one. I believe that this grave malady,

which has tested surgical skill during two thousand years and produced a ponderous literature, which has been dreaded beyond all things by mankind, and has been the source of untold suffering to thousands in every age, is nevertheless a malady which may be exterminated; so far, that is, as it is a painful and dangerous one.

It is my business to-night to deal with stone in the adult—the largest and most important part of our subject; and in speaking of what lithotrity can do, although it is not absolutely powerless for children, being only useful for very small stones, I beg you to bear in mind it is solely a question now of stone in the adult, and that necessarily means stone in the adult of advanced age.

To pursue my subject. I place before you four trays from my cabinet, holding about two hundred calculi removed by lithotrity: the mean age of these two hundred cases is upwards of sixty years; there are very few below forty, many between seventy and eighty. To be exact, there is a tray of sixty-three rather small stones, two trays containing ninety-nine stones of middle size, and a fourth tray containing thirty-five stones of rather larger size; in all, one hundred and ninety-seven. I could have brought you many more, but these will suffice for my illustration, containing as they do several remarkable individual specimens.

Let me now consider the single question—What is the object we propose to attain in lithotrity? I answer, in brief, to reduce the stone to such small fragments that they may pass easily by the urethra, and to do this by exerting as little mechanical action as possible.

We are to aim at applying force to the stone in such a manner as not to injure the delicate canal which must be traversed, and the sensitive organ, the bladder, in which it lies. And the force should be so applied that the minimum of irritation should accrue from the fragments that are made. In fact, the sources of danger in lithotrity are but two in number: injury to the soft parts by the instruments employed; and injury to those same parts from the sharp edges and angular forms of the fragments which are produced by the process. When we have reduced the mechanical action to the minimum of capability to inflict mischief, and have learned to make fragments in such a manner that they shall produce the least amount of irritation, why then we shall have arrived at perfection in lithotrity. Hence it is that I have always endeavoured to work with the fewest and most simple instruments possible, and to reject preliminary injections always, and subsequent washings-out as far as possible.

How far have we solved the problem in question? I answer,

perfectly, in relation to calculi of a certain size. With a calculus of certain weight and dimensions, whether of uric acid, phosphates, or oxalate of lime, say not exceeding the volume of an ordinary nut, a perfect result may be ensured. I call your attention to that tray containing sixty-three stones, and in which, let me remind you, the patients were of a mean age of over sixty years. There was not a single death among those cases. The size I name was not exceeded. And I assert that I have never yet had a death following the operation of lithotrity in which the stone was within the limits of that size. Nor have you any right to expect anything but success with such stones, requiring, say, two sittings, or perhaps three at the most, if you only take care to act with the utmost gentleness.

So far, then, is the problem solved, and triumphantly. But it is another thing when the stone much exceeds these dimensions, and where, not two, but five sittings are necessary for its removal; and, *a fortiori*, danger still increases when the requisite number of sittings rises to eight or ten.

In the next two trays are about a hundred stones of the middle size. At this size, which is that, say, of an almond in its shell, the result is still most excellent, far superior to that of lithotomy, but not a sure success as before. Hence a certain small proportion of deaths was met with—about one in twelve or thirteen cases.

In the last tray of larger stones, the mortality was more considerable, perhaps one in eight or ten cases. With regard to some of the latter, it is quite possible that some of them would have been better cut. No man is always wise, and errors of judgment must be admitted; he is the best who makes the fewest, and marks well his errors, when they are made, for future guidance. After the event, also, with the fresh light of the past on the case, it is easier to know what should have been done. Then there must always be a number of cases in which the choice of operation hangs on the slightest circumstance. There is a wide border-ground, so to speak, which is common to both operations. No man, whatever be his experience can lay down a hard and fast line, and say, "On this side lie the circumstances which determine lithotrity, and on the other side are the circumstances which make lithotomy imperative." The border-ground is much broader than I often like, and there are a good proportion of cases in which it is impossible to predict which operation of the two will be most successful: stones which are just large enough to be crushed, which are certainly not outside of the limits of size for lithotrity, and in patients of a certain class for whom a half-dozen sittings will not tell heavily. On the other hand, the crushing of the

same stone in the sensitive patient with a worn-out nervous system may make too large a demand on his endurance. I do not *know* that. I only say it may; for, after all, the hale and hearty country squire, "who has never been in bed a day in his life," and who never ailed anything until he had a stone, often bears confinement and attacks of irritation less easily than any other patient whatever. Indeed, it is most difficult sometimes to decide; and sometimes the patient who is approached from sheer necessity, with fear and trembling for the consequences of interference, turns out the best of all, and astonishes you with his capacity for getting well. Thus you see the only certainty of attaining success lies in getting the stone to operate on when it is small. And success being absolute then, as I think I have been able to prove beyond all manner of doubt, it follows as a matter of necessity—

That the Diagnosis of the presence of stone in the bladder and of its size is a matter of the highest importance.

I affirm that it is not less important to be capable of finding a stone when small and determining its size, than it is to perform the operation properly afterwards. I might almost go further: I think I shall find you agreeing with me before I conclude, that the diagnosis I speak of is the more important matter of the two; and that I may venture to say that I would rather, for the sake of calculous patients at large, and for the future of lithotrity, have keen diagnosticians than expert handlers of the lithotrite, if I could not have both. Because, as we shall see, all progress depends on the *early* diagnosis; for when the stone is really small, no man worthy of the name of surgeon, and with a fair experience of instruments in the bladder, will fail to crush it safely. You see it is lithotrity that has brought this question of diagnosis home to us. When there was but one mode of removing the stone—when it was necessary to cut from the perineum to the bladder for every stone, no matter how small or how large, it mattered very little whether we made an exact estimate of its size, if we only were quite certain a stone of some sort was there. What, again, did it signify whether it were mulberry, uric-acid, or phosphatic in its character? It was nothing to the lithotomist whether one would crush easily and the other with difficulty or not at all.

Nor is there any real difficulty about making the diagnosis on which I lay so much stress. Nothing is more easy, as I shall soon have to show, if only you follow the right method. Granting me this, and the unrivalled success of lithotrity for small stones, already proved, it logically follows—

That the operation of lithotomy must in future be rejected for all stones which are of moderate size.

Now this is a most important fact, and it is one, I believe,

which has not yet received its full consideration from the profession. For it follows, further, that all those attempts which have been made during the last fifty years, and may still be making, to perfect lithotomy for small stones are useless and obsolete. We cannot require an operation by cutting for small stones. And I am quite safe in saying that the results of that tray, with sixty-three cases of elderly adults without a death, never have been, and cannot be, equalled by any cutting operation whatever.

But it may be said that there are still some exceptional cases of small stones to which lithotrity is not applicable; such, for example, as cases of narrow stricture, preventing the introduction of a lithotrite into the bladder. Perhaps there may be such, but I have not yet lived long enough to find one. During the last few years I have crushed several cases of small stones in presence of narrow stricture, and in this manner: nothing is easier than to dilate temporarily any stricture if you only permit a gum catheter to remain lying in the urethra a sufficient number of days, and this is what I have done in the wards of University College Hospital with the best result. I tie in a very small catheter, and having arrived, after a few days, say at about No. 9, the patient is submitted to chloroform, the instrument withdrawn, a small lithotrite introduced two or three times, the stone crushed, débris removed, and the catheter is replaced. In three days or so the process is repeated, and so on until all is finished. There is no difficulty in thus dealing with a small stone, however resilient be the stricture. Such cases, happily, are very rare, but when they do occur such an operation is usually preferable to lithotomy, for, I repeat, a small stone.

Well, then, these matters being held as proven, lithotrity being answerable of the successful issue of all cases of stone of moderate size, it follows that there are two desiderata, and two only, to bring us to the perfect practical solution of the great question.

1. We want the best operation for the removal of large stones from the bladder.

2. We want the best method for discovering the existence of small stones in the bladder.

Now, the first I do not intend to discuss at length here; that would not be within our limits. I shall assume that for most cases the lateral operation is probably the best. It is no new question, and need not come up again here. But the second is, I declare, a new question. It may seem trite to say so, but practically, I repeat, it has engaged no one's attention at all adequately—that is, compared with its surpassing importance. I find people again and again with small stones in their

bladders which have been overlooked. Not because anyone is to blame for not finding them; first, because the early signs of stone, the evidence of the presence of the small stone, have not been sufficiently considered and taught; and, secondly, because the paramount importance of the discovery did not exist until the power and value of lithotrity in these cases had been demonstrated. Why, I have myself witnessed the sounding of a patient, and heard the result stated in such terms as these:—"I am glad to tell you that you have nothing considerable in your bladder; there may be a small stone perhaps; just possibly, but you have nothing considerable there, so all is well." The patient is congratulated because he has not a stone as big as a hen's egg! But suppose there should be one the size of a bean? Why then the discovery of that stone would actually be a fact of greater importance to that man than the discovery of a big one; because, in the case of the large stone, there is but one thing that you can do, and the issue is doubtful; but find that man's stone when it is small, and you are *certain* to save him. This is a matter I cannot exaggerate the weight of. And it is by following the clue that this gives us, and no other, that we shall arrive, as we certainly shall some day, at the end I have in view—viz., the extermination of stone in the adult.

This brings us to the practical mode of looking for the stone early, and discovering its presence when small. How is this to be done?

First, respecting the mode of sounding. It is absolutely essential to employ a light sound, which can be easily turned in the bladder and urethra. Nothing but a quick and delicate movement will elicit an audible note, or produce a sense of contact, with so small a body as a pea lying in the interior of the bladder. Therefore it is better to have an instrument which will roll easily between the finger and thumb, and not require the wrist or the arm to create the movement. Hence the handle should be cylindrical, like the handle, but smaller, which I originally designed for my lithotrite, and which is now much used here, and is almost universally employed abroad. The beak should be very short, so as to be turned with the utmost facility. To find a small stone the bladder should be empty or nearly so. I prefer a patient to make water a few minutes before sounding, and certainly never to be injected or prepared in any way, which only tends to defeat our object. Let him lie down, with his pelvis a little raised, and then let the instrument gently glide down the urethra; it is five to one, however small the stone may be, that it is just grazed as the beak passes through the neck of the cavity into the bladder. This is perceived easily if the sound is only guided lightly by

the faintest touch of the finger and thumb. Held and guided by the wrist and arm so slight a graze may be unnoticed. If not felt, let the sound make two or three quick semi-rotations right and left; if still nothing found, depress the handle slightly to turn the beak below, close to the neck of the bladder, and make two or three similar movements there. That is where the stone will be found, if there is one, in the nearly empty bladder; and in the same manner a small fragment will be found at the close of a case.

Now look at the sounds which were formerly used—heavy, large, with considerably curved and long ends, like those of an ordinary catheter or nearly so. No doubt you might find a large stone with such a tool, but a small one never, except by accident. I have repeatedly demonstrated, not only here, but in Paris, the presence of a small calculus, or fragment smaller than a pea, by producing an audible note from it by means of the small sound with cylindrical handle, passing a lithotrite to the spot immediately afterwards, and withdrawing the bit uncrushed as a proof. Pray permit me to say that I don't adduce this as any instance of fancied dexterity on my part—far from it; for you see it would defeat my very purpose to do so: it is merely an illustration of what is possible equally for you as for me, if only you pursue the rational system of seeking the small stone with an appropriate instrument. If it be not so I will sit down and give up my case in favour of lithotrity, because it is altogether a worthless operation if it is not capable of removing the whole stone. If lithotrity means only crushing a stone to pieces, and does not mean also making sure to remove the last, then by all means let us do lithotomy, and nothing else but lithotomy. Lithotrity is quite equal to its object in nineteen cases out of twenty. I do not mean to say that a case will not now and then happen in which the last fragment will not yield to our search, and may give some trouble. In all operations, however perfect, some *contretemps* may present itself. But I contend that it is very rare to miss the last piece. Seek it in an empty bladder with such a lithotrite as this; no danger whatever to the bladder exists. It is useless to seek it in four or more ounces of water, which is simply "looking for a needle in a bottle of hay;" currents and counter-currents are produced in all that water by the opening of the blades and the action of the bladder, which keeps your little bit in a state of constant movement.

Secondly, the early signs and history of vesical calculus are to be carefully noted. This brings us to what appears to me a very striking and suggestive question,—How is it that the existence of a calculus in the bladder, a product almost always of slow growth, and giving abundant signs of its presence, can ever

attain anything beyond a certain size without being discovered? That it does so is too true; but that it should ever grow to be of large size is to me astonishing. I assert that more than half the stones I operate upon are found in cases in which no suspicion as to the real malady has arisen until the sound has been employed. Now, with the utmost deference to others, and only after the acquirement of a profound conviction on my part, I venture to say that it is my belief that the early signs of calculus are not generally sufficiently recognised. In the whole course of my experience I have not met with more than two or three cases in which the obvious early signs of calculus were absent. To me they appear quite unmistakeable. They may be present, or nearly so, in cases in which no calculus exists; but when these signs are present, then always ought the sound to be used. So far have we been from recognising who are the real calculus patients that we find it stated in our classical works on the subject that stone is most prevalent in children. Not at all. Stone is uncommon in children compared with its frequency among elderly adults. I am quite aware that, in most large hospital records, half the entire number of cases are found below the age of puberty; and it is that which leads to such surprising statements about the small mortality of the lateral operation, as I have before alluded to. But then among the poor stone is comparatively frequent in children. Among the well-to-do it is very rare to find a juvenile case at all. The latter class, however, furnish it abundantly at the other end of life, and here it is that the bulk of stone cases is to be found.

And what is the ordinary or typical history of a stone case? I speak, as anyone, of course, may understand, of uric acid and oxalate of lime. The phosphates are mostly of local origin in a bladder incapable of emptying itself, and belong to another category. You will find a healthy-looking man with good family history as to longevity, but mostly tainted with gout, one or two cases of it existing, antecedent or collateral, or in its absence some record of gravel or stone in an ancestor. At middle life he finds uric acid in his urine, as a brickdust deposit, more or less persisting. Soon after, a small bit of gravel passes, with or without a marked attack of renal pain; if the latter, he is at the time much relieved by medicine; but often no special treatment or regimen is adopted at this critical point in his life to check the tendency now fully developed; so, after an interval, another and another pass, and then no more for a few months; and although some little suspicious symptoms appear, they are thought very lightly of, especially as he has not during the last nine or twelve months passed any gravel as he used to do. Whereupon he congratulates himself and is congratulated —not prudently; and the suspicious symptoms are credited so

often "to that little weakness of the bladder which all people have as they get onwards in years." Delusive axiom! But what are these suspicious symptoms? Not very marked, but ample to render almost certain to the experienced observer that that interval of freedom from passing gravel only marks an advancing stage of the malady, and shows that the gravel is now too large to pass the urethra; that it is in the bladder and is growing by accretion, consuming surplus uric acid for that purpose day by day. For on inquiry you will find that the frequency of micturition is greater by day, during movement, than by night, during rest—condition altogether contrary to that "weakness of advanced years" (prostatic hypertrophy) when the frequency is almost always greater by night than by day. You will find a slight pain—a mere passing sting—is mostly present at the close of the act of micturition and in the end of the penis, while in the "weakness" the pain, if any, is from distended bladder, before micturition, and is relieved by the act. Next, it will probably be ascertained that some day lately, after an unusual walk, or it may be after an hour or two in the saddle, a little blood was observed in the next urine; soon forgotten, or, if named, was followed by the recommendation not to do it again, but which unhappily aroused no suspicion of the true cause; so, not being done again, the occurrence did not reappear, which again comforts everybody. Well, after listening to such a history, I am always morally certain that one or two small stones exist, and of course the sound is introduced at once, and almost invariably a small stone or more are discovered. No anxiety need arise, and the patient may now with reason be congratulated, since a small stone is certainly the safest solution of his symptoms; for, as I have already said, the malady for the most part occurs in people of otherwise good health and strong constitutions. I have no hesitation, then, in saying that from fifty-five to seventy-five years of age is the favourite term for calculous diseases, at all events in this country.

I now think I have fairly proved that the operation of crushing the stone is safe and successful for all small stones, and I think I have also proved, or have gone far to do so, that a stone may be always found when it is small. It follows then, if you admit these things, that lithotrity must be the future operation for calculus in the adult.—*Lancet*, *Dec.* 20, 1873, *p.* 867.

76.—ON THE DETECTION AND REMOVAL OF VESICAL CALCULI

By William Donald Napier, Esq.

[Three points with regard to vesical calculus are obvious. 1. The difficulty of early diagnosis. 2. The danger to life involved by

the operation of lithotrity as now practised. 3. The impediments to convalescence consequent on its successful performance. The instruments described by Mr. Napier are we think of great value, especially as by their simplicity of mechanism and manipulation, they will do much to ensure efficiency in the hands of practitioners who have not had any very special experience.]

To render a doubtful result to investigation conducted with ordinary care more exceptional than hitherto— and I imagine that it is in the first stages of the disease that the percentage of uncertain issues is by far the greatest—it seemed necessary to devise an instrument of peculiar sensitiveness, and capability of application to minute objects. Whether the one before you will fulfil these conditions in all instances, remains to be proved. It has not failed me yet in the experiments to which it has been subjected. I believe it to be unique in its mode of action, depending, as it does, upon ocular instead of oral proof; and I cannot help hoping that, by reason of its exquisite susceptibility of impression, immediate manifestation of the existence of a foreign body may follow with absolute certainty its introduction into the bladder.

The Calculus Detector, for that is the name I have given it until a better can be found, is precisely similar in form to the ordinary sound. It is composed of steel up to the commencement of the curve, where it is reduced in diameter, and a coating of pure lead is cast round the smaller part, of sufficient thickness to render the surface perfectly smooth and even along the entire length. The leaden extremity is then polished as highly as possible with a leather, and rendered keenly sensitive to the slightest contact with any rough or hard substance, of which it would be now found to bear visible trace. On some of these instruments I tried to increase the susceptibility by dipping the leaden end in a weak solution of nitrate of silver, or what is better, chloride of platinum, which produces a black pigment on the surface; but further experiments have almost convinced me that this preparation is unnecessary, and that the pure lead is perfectly efficient. The instruments that I showed before were coated with pewter, on which the use of lead is a great improvement, by reason of its readier impressibility.

Before the introduction of the detector, it should be carefully rubbed with a leather, and submitted to the test of a magnifying glass, so that no slight scratch or indentation previously incurred should, on its withdrawal, mislead the operator, who will find it satisfactory again to apply the same means for discovering the marks which contact with a hard substance may have produced during its employment in the bladder.

With regard to the other instrument, which I call the Calculus Extractor, I may premise that I incline to confine its use to the removal of small stones—I trust that the day of large ones is at an end—and fragments of stone; and this because I do not myself believe that lithotrity should ever be resorted to when a stone has from any cause, such as absence from available medical assistance, or the dread of the sufferer to submit to the necessary test, been allowed to attain to large dimensions. Then the patient's chance for recovery must lie in lithotomy.

To be as concise as I can, I shall simply describe it as an instrument intended to withdraw from the bladder either a stone intact, or crushed into fragments, so enveloped in a delicate elastic wrapper—I use the term advisedly, for as you will see, it literally folds itself round the substance in question—that injury to the passage will be simply impossible. It consists of a soft India-rubber tube, terminating at one extremity in a funnel-shaped orifice, greatly resembling the ordinary convolvulus flower, and it is capable of containing a calculus or portion of calculus of considerable dimensions. When it is inserted into the bladder, its form and position render it the receptacle of such foreign substances as would from the position of the patient and the flow of urine determine towards the natural outlet. This receptacle, enclosing such bodies in the folds of its delicate membrane, and permitting itself to be gently withdrawn, retains them in its elastic envelope, and protects the urethra from direct contact with them.

After bringing it to the state of completeness in which you now see it, I was very much and very long perplexed as to the best means of introducing it into the bladder; and it was only after many trials and as many failures that I was able to see my way to the accomplishment of this, without which my instrument would be useless.

My first idea had been to insert the convolvulus-shaped cup folded in the form and manner of a bud, and retained in its position by some adhesive substance that should permit it to expand after its introduction. But there were several objections to this. At last it struck me that, inserted in a fine silver cannula, it would be entirely at the direction of the manipulator; and I was successful in reducing my idea to practice; but then I was met by a difficulty that I feared at one time would be insurmountable. How was it possible to avoid the trouble to which the insertion of a cannula open at the end could not fail to give rise? An artificial point appeared to be the only solution; and it seemed to me that cocoa-butter was possessed of a peculiar fitness for the purpose; for this reason: for a hard dense substance, its dissolubility under moderate heat is, I believe, quite without parallel. Subjection to a temperature of

96 degs. dissolves such a portion of it as my purpose requires with curious rapidity—in as nearly as possible one minute. There is besides the additional advantage that it serves as a lubricator in its passage to the bladder. But it is not in connection with my instrument alone that this application of it will, I hope, be found to be a profitable discovery, but in every operation where it is expedient temporarily to close the mouth of a catheter or cannula. I mean that I consider the cocoa-butter point *per se* worthy of attention.

I would now briefly suggest to you a further adaptation of the principle and form of the Extractor that may be found useful. As a self-retaining catheter, I cannot help thinking it would prove as simple and effective an appliance as could be used; and I know that the increased demand for these instruments of late has called forth various improvements and emendations in the means both of introduction and of retention.—*British Medical Journal, Dec.* 20, 1873, *p.* 720.

77.—MR. GUTTERIDGE'S OPERATION FOR STONE IN THE BLADDER.

Mr. Gutteridge's scheme consists of two parts, viz.—to use his own words,—"of a due combination of incisions of parts ascertained by extended experience to be capable of being safely severed; and of implements with which the successive stages may be most surely and with least danger accomplished." The patient was a boy sixteen years of age, a native of the Black Country, and was submitted to operation without being anæstheticised, as Mr. Gutteridge has a strong objection to chloroform or ether in lithotomy. Kneeling before the patient when in the lithotomy position, and with his eye thus in a line with the perineum, the operator first passed the staff, which he handed to his assistant; he then made very firm pressure with the fingers of his left hand upon the anus, thus dragging the skin of the left side of the perineum, with the lower extremity of the gut, well over towards the right tuber ischii. He next rapidly plunged the scalpel into the groove of the staff at a point a little to the left of the raphe, and on a level with the anterior extremity of the tuber ischii, and then, without running the scalpel along the groove, he made it cut its way through the soft tissues. Next he passed the beak of the cystotome into the roughened groove, and, with the cutting edge directed a little upwards from the horizontal direction, he passed it onwards through the prostate and then turned the edge downwards and brought it out at the lower extremity of the skin-wound. This done, he passed his left index-finger into the bladder, felt

the stone, then withdrew the staff, and passed in along the finger a grooved conductor, much like a narrow blunt gorget, and along this, after withdrawing the finger, he guided the forceps, only one blade of which was *in* the groove of the conductor. The stone was seized and at once withdrawn, and the operation thus completed.

It will be understood from this account that the method differs from that adopted by other surgeons in that—(1) no anæsthetic is administered; (2) that in dividing the structures in the ischio-rectal fossa the scalpel is thrust by one plunge into the groove of the staff at the point of the anterior extremity of the wound, and that owing to the traction made on the anus, the incision through the skin, when the parts are relaxed, is seen to extend over the tuber ischii; (3) that the incision whereby the bladder is opened is semilunar in shape, and describes a curve constituting an arc (as Mr. Gutteridge states it) of 100 degrees of a circle; (4) that the forceps are guided into the bladder along a grooved director instead of along the left-index finger. Mr. Gutteridge spares, if possible, the membranous portion of the urethra, and lays open only the prostatic part. His aim is to make his knife enter the canal at the apex of the prostate gland.

For these modifications in the procedure special instruments are provided. These were recently shown to members of the British Medical Association, at the forty-first annual meeting, in August last, a description of which Mr. Gutteridge has himself given as follows:—"A table constituted of a fixed frame, on which is a sliding top, whereby the patient may be firmly fixed and kept symmetrically in all respects, and yet through which, by means of a simple screw action, the position of *tight-up trussing* may be speedily relaxed, and the pressure on the respiratory and venous systems lightened, so that in a protracted operation rest may be given and renewed efforts made, by intervals, at the operator's pleasure—a resource in the aged and the fat of the very first importance as respects the sustaining the patient's power of endurance without fatal congestion. The manual instruments consist, first of a staff, with handle capable of being used by the operator and the assistant at the same instant of time, and with a furrow for the knife to traverse, coiling slightly, and ruffed so that the holder of the knife may have a positive assurance of the knife and staff being in real contact throughout the course of the knife, until it is entered into the bladder. Secondly, an implement consisting of a handle shaped for the finger and thumb, with scalpel mounted at one end; and at the other a beaked knife—properly a cystotome—fixed at a right angle to the scalpel on the axis of the haft, so that one instrument may effect, by a move of the finger and thumb, the two distinct incisions, the external one and that of

the prostate. Thirdly, a conductor for the forceps conformable in respect of size to the operator's left forefinger. Fourthly, forceps with supplementary handles by which the stone may be compressed with hair-light pressure while increased force is brought to bear on the main arms of the forceps; admitting of the separation of the process of compression and traction; as well as relaxation of the hold of the stone, for the purpose of adjustment of the forceps to the stone, or gentleness of pinch, in case of the stone being of friable material."

The operation for stone performed with these instruments and in this way Mr. Gutteridge would desire to have known as "lithotomy made speedy and safe." Certainly in his own hands, when performed as we saw it, it is rightly so styled. It was done in a remarkably short time—less than half a minute,—and every step in the operation was executed with rapidity and precision. The boy has gone on perfectly well since the operation, and within forty-eight hours afterwards the urine was all voided by the natural passage. This, as is well known, is an unusually short time between the operation and the safety of the patient, which is secured by the flow of urine along the urethra. Yesterday, the eleventh day after the operation, the patient returned to his home in Staffordshire, perfectly cured. We sincerely hope we shall have other opportunities of seeing Mr. Gutteridge cut for stone.—*Medical Times and Gazette, Nov.* 15, 1873, *p.* 548.

78.—CURRENT VIEWS ON THE TREATMENT OF STRICTURE OF THE URETHRA.

By WILLIAM S. SAVORY, Esq., F.R.S., Surgeon and Lecturer on Surgery at St. Bartholomew's Hospital.

[The most current view of the nature of stricture is simply a mechanical one.]

That stricture of the urethra is, to all intents and purposes, a constriction of a portion of the canal, due to thickening and contraction of a deposit in its walls, just as any tube or pipe may be blocked up, to a greater or less extent, by foreign matter; and the worst of this is, that it at once suggests to him the notion of treatment by purely mechanical means; that the obstruction must be overcome by force, either gradually and more cautiously by the repeated application of less force, or rapidly and violently by the sudden application of much force. Stricture of the urethra is too often regarded as a simple physical condition, to be treated only by physical means.

The point I am anxious to impress is this: this plan—the

usual plan—of treating stricture is founded on a misconception. Excluding from consideration now those cases of so-called stricture in which there exists no structural changes, even in the simplest cases of organic stricture, the condition is hardly ever, practically never, a purely physical one. With the structural change, there is associated an active morbid state, either some inflammation or congestion of the parts, and almost always more or less of spasm. The evidence of this seems plain to those who will read it.

How otherwise can one account for the varying size of the stream, even when permanently diminished? Very few instances of stricture occur in which, upon investigation, it does not appear that the stream of urine varies much in size; that, at certain times, and under favourable circumstances, the urine is passed much more easily and quickly than at others. This fluctuation in the size, and other characters of the stream, which is so very common in cases of stricture, cannot be due to any permanent alteration of the canal, but must be referred to some temporary cause which is capable of modifying its size and shape. Then the greater facility with which instruments may be introduced at some times, and under certain circumstances, than at others, irrespective of skill; and the speedy improvement which follows measures that can hardly, in so short a time, affect a permanent stricture—such as rest, warmth, aperients, and opium—can only be explained in the same way. And again, would an organic stricture, in a passive state, account for tenderness and disposition to bleed of the portion of the urethra which is the seat of the obstruction?

So, too, is evidence of the same kind offered by the effect of removing the pressure of the urine from behind, as by tapping the bladder. This is often at once followed by partial relaxation of the contracted portion of the canal. Beyond the immediate relief which is afforded by the operation, this appears to be the great advantage which is derived from puncturing the bladder or urethra behind in retention of urine from stricture. The urine is no longer impelled against the obstruction by straining, and the urethra is placed at rest. So also the evidence of the effect of enlarged prostate. Brodie remarks, "Where a simple chronic enlargement of the prostate gland supervenes on stricture of the urethra, the latter usually becomes less liable to spasm, and is more easily dilated, and altogether more tractable than it was before." Because now the pressure of the urine, which was a constant source of irritation, is intercepted by the prostate.

How otherwise can one explain the fact of complete retention of urine suddenly supervening upon stricture, with the fact, often verified after death, that the degree of organic con-

traction will not account for the degree of obstruction which exists?

Thus, it may be understood how the controversy about impermeable stricture has arisen. Much discussion has been expended on the question, whether such a condition as a really impermeable stricture ever occurs. That the canal of the urethra is ever so permanently closed and sealed up by organic stricture as to be altogether impervious to urine, may be very well denied; but no position can be less tenable than that taken by those who argue that, where urine can pass out, an instrument may be passed in. In some cases, indeed, neither urine nor instrument can pass; but, even if the former can somewhat freely escape, it by no means follows that the latter can be passed through. This is a very different matter; for, setting aside other considerations, everything may depend upon the presence or absence of spasm or muscular contraction. There may be a passage for the urine, but it may be closed against an instrument.

Therefore, in the treatment of stricture, the employment of instruments should never be an early, only a late resource. Of course, I am not speaking now of retention of urine in consequence of stricture—that is another subject—but of the direct treatment of stricture of the urethra. In these cases, I submit that we should turn our attention more to the cause and less to the effect. The new matter, the lymph, which has been deposited or formed, and has subsequently contracted, causing stricture, is the result of a morbid process, which has either passed away, or which is still active, or lingers.

If such action be still present, what can be conceived more mischievous than irritation by an instrument of any kind? What can be more opposed to all that are recognised as sound principles of surgery? What would be done in an analogous case elsewhere? Would it not be deemed of the first importance to remove all causes of irritation—to obtain as far as possible absolute rest—to meet at once and directly the cause of the mischief, and assuredly not by rash meddling with the effect to aggravate the cause? Surely this is sound in reason, and I will venture to say supported by the results of the practice founded on it.

Foremost among the means at our command is rest—absolute rest in the recumbent posture. I do not pretend to say precisely how long this should be persevered in, or how strictly it should be enforced in every case; but I venture to assert that in the treatment of every case of stricture it should have a fair trial before any other means are resorted to. I do not think mere avoidance of business or confinement to the house sufficient; it should be continuous rest in the horizontal

posture. Much good must be attributed to the effect of position. In short, the patient should be kept in bed, for this not only secures rest in the right position, but uniform warmth, which is a great point also. No doubt, many patients with stricture will object to this, just as surgeons neglect to enforce it, because its full value, and all the cost and trouble it saves, are not recognised, but I believe that, in proportion as experience is gained of the practice, so will it be esteemed—that those who carry it out most strictly and perseveringly will be best pleased with the result.

Beyond the all-important means of rest and recumbency, assisted by warmth in the form of baths or otherwise, there are remedies of great power in allaying irritability. Opium, which in these cases is so often valuable, as a rule perhaps is best administered, as laudanum, by the rectum; but this, or some other form of it, whether solid or liquid, may be given, when required, by the mouth, or morphia may be injected under the skin. Belladonna, too, or its alkaloid atropine, is sometimes useful, especially when locally applied; but, as a rule, I think they bear no comparison in efficacy to opium.

Active aperients also are often strikingly serviceable. The urine, which up to the period of their action had been obstinately retained, will sometimes pass freely during a brisk action of the bowels. The escape of urine, which is often simultaneous with the action of the bowels, and usually then most free, is a fact which should be always borne in mind. And it may be remarked that the success which attends the use of such remedies as those which have been indicated, is an additional proof, and a strong one, of the nature of the case. But, except in extreme and urgent cases, the use of drugs and of all active remedies may well be dispensed with, and reliance placed on simple measures of hygiene, such as wholesome food, warmth, and, above all, rest for a while even in bed.

But suppose a case in which all morbid action in the way of cause has passed by, and in which nothing remains but the effect, the thickening and contraction, what then? Now, in the first place, I say—and here, no doubt, I shall be at issue with many—that such a case in actual practice is, to state the least, extremely rare; that a troublesome stricture, due simply to a mechanical cause, apart from all morbid action of any kind, is to be regarded as a clinical curiosity; that this condition of things, which so often exists in idea, is hardly to be found in fact. But assuming it to be, even then I would repeat, still keep instruments in the background until other measures have been fairly tried. No one, I think, who has properly tested the value of these measures will be disposed to treat them with indifference in any case. Is it so anomalous an

event for this new matter, this lymph, this outcome of inflammation, or of some kindred morbid process, to yield to time, when the parts thus for awhile hampered in their function are placed under conditions favourable to recovery; when causes provocative of still further mischief are carefully excluded? At all events, I would repeat the statement that these simple measures seldom if ever fail to effect improvement of the stricture. Of course one cannot pretend to measure in a given case how much, if any, active mischief may still be lingering there. In many cases it may be very little, in some possibly even none. But this I can affirm: I cannot call to mind a case in which a stricture that gave trouble was not relieved by rest, recumbency, and the other means which have been mentioned. If this be so, should it not form an essential and primary part of the treatment of stricture? Should not the most be got out of these measures before we resort to instruments?

If it be urged that this would be waste of time, which may be saved by employing instruments at once, it may be replied that chiefly out of such mischievous practice have arisen such convictions as those to which I referred at the commencement of this paper. But then it will be argued that after all you come at last to instruments to accomplish the cure. Not always—very often not. I do not pretend that, under the sole influence of these measures, which are simply hygienic, all traces of stricture usually disappear; but the patient is very often, and sometimes speedily too, restored to such comfort, he is so relieved from every trouble, the stream of urine flows so naturally, that he is well satisfied with the result, and declines any further interference.

But by these means you have not thoroughly cured his stricture; you have given him only temporary relief, and his troubles may at any time return. That will in many cases depend on himself, and the care he takes of his health. No doubt in most cases of severe stricture these simple means are only palliative. Frequently, indeed, they will succeed so well that the patient himself is no longer conscious of anything amiss, yet the passage of an instrument will still detect an abnormal condition. But supposing you fall back on instruments, what then? How much more than this can you do? Dare you, in the majority of cases of severe stricture, promise by their aid a complete and permanent cure? Can you do more than restore a patient to a condition of comparative comfort, yet still with liability, under unfavourable circumstances, to relapse?

The sum of my argument, then, amounts to this: while I am not suggesting that instruments are never to be employed in the treatment of stricture, I venture to affirm that they have been and still are, grossly abused; that an untold amount of

mischief is perpetrated by their abuse; that the routine employment of instruments in the treatment of stricture is in the highest degree unscientific and improper; that, in a word, in the treatment of stricture, instruments of any kind should never be employed in any way except as a last resource.—*British Medical Journal, Dec.* 20, 1873, *p.* 713.

79.—ON EXTRAVASATION OF URINE.

By WILLIAM S. SAVORY, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

Extravasation of urine occurs in two forms. The one, severe and rapidly destructive—the acute, as it may be called; the other, slower, far more gradually and indirectly mischievous—the chronic.

The first, where the escape of urine and infiltration of tissue are sudden and abundant, is usually the result of a fall or blow upon the perineum, whereby the urethra is lacerated. The immediate effect of the injury is to produce ecchymosis—swelling and discolouration of the perineum, and more or less of the scrotum, with perhaps the escape of blood from the orifice of the urethra. But the patient soon finds that he cannot pass water, and the attempt to do so is followed by a very rapid increase of the swelling of the perineum and scrotum, until the former becomes extremely tense and the latter enormously distended. Then, if no relief be given, the infiltration and its consequences extend forward toward the penis and upward over Poupart's ligament on the abdomen; its course being determined, as everyone knows, by the relation and connexion of the superficial and deep fasciæ. Now such mischief is marked by very striking local and constitutional effects. Locally, by rapid destruction of the infiltrated cellular tissue—decomposition, suppuration, and sloughing; generally, by severe constitutional disturbance or fever, which rapidly assumes a typhoid form.

Now what is to be done in such a case as this?

[All the mischief being due to the presence of decomposing urine, which is confined by the skin, our first step should be to make free incisions. The gush of stinking urine is succeeded by an abundant drain.]

There is no difference of opinion about the necessity and efficiency of free incisions, and nothing can be more gratifying than to observe their effects. A patient, beforehand, may appear to be almost moribund—in a state of extreme depression, with a pinched and anxious face, a feeble running pulse, a dry and brown tongue. A glance at the scrotum tells the tale. You give him brandy, gently raise and separate still further his legs,

and lift him to the edge of the bed; in a few seconds the incisions are made, and in a few hours after he is a different man. The relief has been complete; he rallies rapidly, and usually does well.

[An important question is, whether a catheter is to be retained in the bladder or not. This the author decides in the negative, because it may cause much irritation, and even if it does not do so, it does not fulfil the object intended, as the urine still continues to flow by the side of the catheter and through the wounds, although in lessened amount.]

It is urged, the escape of urine by the rent in the urethra prevents its closure. Now, considering the kind of injury the urethra has sustained—not a clean cut, but bruising and tearing, —I doubt very much whether the passage of urine over it is for some time more unfavourable to repair than the constant presence of an instrument there. But be this as it may, the objection to the retention of a catheter rests on other, clearer, and far stronger grounds. It is a fact that when an instrument is retained in the bladder all the water does not continue to escape through it, but some will at length inevitably find its way along the outside of it—between it and the urethra, and this urine, mingled with pus from the suppuration of the canal provoked by the catheter, escaping by the wound in the urethra, must, at least, be mischievous enough to countervail any advantage that could accrue from the instrument.

As a matter of fact, I have often seen a catheter employed in these cases, but never, I must say, without, in my opinion, mischief. Given a case of rapid extravasation of urine, such as I have sketched at the commencement, when the surgeon has made incisions into the cellular tissue requisite for the escape of the fluid he has done enough in the way of manual interference. No farther measures are at the time called for beyond poultices or fomentation with antiseptics locally, and nourishment, with stimulants if needful, and perhaps opium. But what will happen? The patient, in all probability, will rapidly rally; urine will escape freely by the wound or wounds in the perineum; there will be an abundant escape of urine and pus, which will quickly become less fetid; and by and by the separation of sloughs of cellular tissue, extensive as the case had remained long without relief. Then the wound contracts and heals, as a rule, completely, though occasionally a fistulous track may for a while remain. As the wounds close in, the urine returns to its natural channel, and at length the patient passes all his water in the natural way. But often, very often, with considerable obstruction, and the introduction of an instrument at this period always discovers a contraction—a traumatic stricture at that part of the urethra which was injured. Now the time has

arrived for the dilatation of this stricture, and, if not before, as the stricture is dilated, any fistulous apertures will usually close.

But of course the question will be pressed here,—Could not this contraction of the urethra have been prevented by the retention of an instrument in the first place, and could not much time have been saved thereby? For my part, I say, No; and as the whole matter in question turns upon the answer just given, it may be worth while to consider it more fully.

Though the urethra may be much bruised and very badly hurt by the injury, yet, for the most part, it exhibits a remarkable power of repair. Injuries which have destroyed the urethra in its whole circumference, or damaged it to an extent which may be measured even by inches, are yet repaired in such a way that the continuity of the canal is restored, although the calibre is of course contracted; so that any notion that the constant presence of an instrument is needed upon which the restored portion of the canal may be moulded can hardly be the result of clinical observation. But then again, Could not the contraction—the stricture—have been thereby prevented? No. Because this contraction—assuredly the worst part of it—is a subsequent process. To retain any instrument continuously for several weeks is of course, as a rule, out of the question. Even supposing it to be retained from the first for two or three weeks—and surely this is a liberal assumption,—still there will persist the most inveterate tendency to contraction long after this, and the hardest part of the battle has still to be fought. In fact, I have not found, to say the least, more difficulty in introducing an instrument and dilating the urethra in those cases where the canal has been left to itself until all disturbance has subsided, than in others in which frequent attempts had been made to maintain its patency from the first. In short, do what you will in these ugly but interesting cases, you must almost inevitably have, by and by, the most obstinate and inveterate form of stricture to contend with: and I cannot but come to the conclusion that you will save your patient much irritation and distress by not attempting to oppose the contraction of the urethra until all active disturbance, local as well as general, consequent on the injury has subsided. I repeat again, that the contraction, which is your chief difficulty, occurs late in the case. The difficulty of introducing an instrument early depends, for the most part, on the swelling and the interruption of the continuity of the canal. The contraction which constitutes stricture is a process, remember, of the material of repair—of the cicatrix, in short; and is there the shadow of justification for the idea that a cicatrix in the urethra follows a law of its own, altogether different from those that prevail elsewhere? And, depend on it, the urethra during its repair cannot be

disturbed with impunity. Therefore I submit that the practice, in these cases, of introducing and retaining at the outset an instrument, requires something more to justify it than the vain hope that we can by this means prevent the formation of stricture.

No strict rule can of course be laid down as to when the dilatation of the resulting stricture should be commenced. The best test for this is the degree of difficulty there is in emptying the bladder. While the urine can escape without distress there is no urgent need for interference; and when doubt arises, the examination of the urethra by an instrument will greatly assist you in your decision. If an instrument can be passed into the bladder, so much the better; and when you will repeat this proceeding, or whether you will adopt some other, will be determined by the effects of the first trial.

But now, supposing all immediate mischief due to the extravasation to have subsided, and, as the wounds in the perineum contract, the patient to experience great and increasing difficulty in passing urine; and supposing, under these circumstances, that you fail to introduce any instrument,—what is to be done? Surgeons will answer this question very differently, but you may make up your mind to this: that whatever plan of treatment you adopt, your practice will be beset with difficulties; and all your judgment, care, tact, and skill will be required to overcome them. You may do many things and much mischief; if you avoid this, and succeed in enabling your patient to pass water in a natural way without discomfort, you will have assuredly done very well.

You must, if you can, establish the route from the penis to the bladder; and for this purpose I know no better plan on the whole, than the following:—The patient is placed in the same position as for lateral lithotomy, and an instrument of moderate size is passed through the urethra as far as practicable, and there firmly held. Then an incision is made in the median line of the perineum down upon its end, and the resisting structures in front of it are cautiously divided. Then through this opening an instrument, such as a long probe, is passed onward to the bladder. Now this is undoubtedly the great, at times the almost insuperable, difficulty of the operation. To discover the track of the urethra, now converted into a fine tortuous fistula, even from the perineum, with a probe is no slight feat; temper, patience, delicacy of touch—large demands will be made on them all. I have seen this operation done many times; often it has seemed as if the operator would, after all, fail. I have known him to fail; but, in my experience, with very few exceptions, the bladder is at length reached. Then the remaining steps of the operation are comparatively easy; either a catheter passed through the penis is, directed by the probe, carried on

at once to the bladder, or a larger instrument is previously passed from the perineum guided by or over the probe; its passage assisted, if need be, by the further division through the wound of any resisting structure. The result of this operation is, that an instrument is secured in the urethra throughout its length, retained there so long as it can be borne, and afterwards an instrument is passed at intervals to maintain the size of the canal.

But it is argued—Why not do at first what you have been compelled to do at length? Why not, when you give escape to the extravasated urine, assuming, as is often the case, that an instrument cannot be passed without—why not at once seek for the further end of the urethra from the perineum, and then pass and retain an instrument. Why? Because, in the first place, it does not follow that such a severe step will ever be required. You cannot tell at first to what precise extent the urethra has been injured, or to what degree it will be restored. Nay, even if you examine the injury through an incision in the perineum, you cannot calculate the extent to which it will recover; you cannot—and this is the point—tell whether at length you may not be able to pass an instrument by the natural course, and so happily—as in almost every case you may—avoid the risks of the operation altogether. But waving this objection, I contend that you undertake the operation afterwards under far more favourable conditions than at first. Your difficulties are lessened because, although the parts are still in an unnatural state, you are not further embarrassed by the great distension due to recent effusion. But, above all, your patient is in a far better state for so trying a measure. Anyone who is the subject of rapid extravasation of urine is of necessity very ill—must be ill for some time to come—is, beyond all question, in a very unfit state for any operative proceeding, especially one which in all probability will prove tedious and exhausting. I say, therefore, that by adopting this proceeding at the outset you are doing, under almost the worst possible condition of your patient, that which might never have been required. Furthermore, the difficulty, the anxiety, and even danger of retaining an instrument, through his subsequent illness, may be so great as to compel you to remove it before the parts have closed in and consolidated, and so in a short time the urethra is no better off than if it had been left alone.

For these chief reasons, then I prefer to wait. I do not think that thereby I lose time; on the contrary, I am convinced that in the end I gain it, for you see, when the immediate effects of the injury have subsided, the parts are in a condition to recover much more directly from any operation.

The other form of extravasation of urine, where the escape

is far more limited and the infiltration very gradual, produces very different effects. Here there is no acute suppuration and sloughing; but the cellular tissue, starting from the perineum at the point of leakage, slowly, at times almost imperceptibly, becomes thickened and brawny, and the skin, too, dense, rough, tuberculated, and reddened. The infiltrated, thickened, and brawny tissue extends, involving in its course, first, the whole scrotum, which may become enlarged to several times its normal size; and then the penis, which from the swollen state of its integument and cellular tissue becomes not only much enlarged, but often curiously twisted and otherwise misshapen, the prepuce especially sometimes being so twisted and tucked in at the orifice that it is very difficult, or even impracticable without slitting it up, to find the meatus of the urethra. When matters have advanced to this stage the appearance is very characteristic, and the nature of the case is revealed at a glance; but at first, when the infiltration has just begun, or when it is very trifling—for it is easy to understand how there may be all degrees of it,—the signs are obscure, and a careful examination is needed to establish the fact. More especially if there be one or more fistulæ in the perineum, it often requires some tact to distinguish between the surrounding condensation of tissue which is the inevitable result of their existence, and that which is due to the occasional escape of some urine beyond their limits.

Now, this form of extravasation, far less urgent than the other, does not call for such prompt and decisive measures. It is almost always consequent on long-standing and neglected stricture, and is, therefore, unlikely to occur, to any considerable extent except in those who are placed beyond the reach of ordinary care and attention. And thus in these cases one is often struck by the signal improvement which rapidly follows simple rest in a hospital bed, and the removal of all those injurious influences to which such patients are accustomed to be exposed. In many of these cases such simple measures of hygiene will suffice, not merely to stay the spread of mischief, but even to enable the tissues involved to recover their healthy state. But in others, again, a farther step must be taken—the dilatation of the stricture. Whether this is immediately necessary or when it should be attempted must be chiefly determined by the condition of the stricture itself, by the mode in which the urine is passed and the size of the stream, and by the result of examination. But of those cases in which the stricture needs to be, without delay, dilated, very few remain which call for farther measures. Unless there are signs of the accumulation of fluid in the cellular tissue, incisions cannot afford any material relief. The brawny tissue may be here and there

divided; but one is too often disappointed at the small advantage of any kind which is gained, while they sometimes bleed actively, and these patients are so frequently broken down in health that they have no strength to spare, and their wounds are very reluctant to take on healthy action and heal.—*Lancet, Jan.* 17, 1874, *p*, 79.

80.—A CASE OF RETENTION OF URINE FROM IMPASSABLE STRICTURE,

RELIEVED BY ICE IN THE RECTUM, AND THE STRICTURE SUCCESSFULLY TREATED BY CAUSTIC POTASH.

By W. F. Teevan, Esq., B.A., Surgeon to the West London and St. Peter's Hospitals, London.

For all practical purposes, strictures of the urethra may be divided into two kinds, the Passable and the Impassable; and the former are as easy to treat as the latter are difficult. An attempt to relieve retention of urine from an impassable stricture is not always successful, and an operation has sometimes to be resorted to. Any remedy, therefore, for this complaint which is harmless in its operation and speedy in its effects commands our attention. I believe M. Cazenave, of Bordeaux, was the first to introduce into practice, some quarter of a century ago, the plan of treating retention of urine by plugging the rectum with ice; and if the method is only half as successful as he claims it to be it will prove a valuable auxiliary to the surgeon. When called to a case of retention, whatever may be its cause, I always endeavour at once to pass an instrument; and I only resort to other means when foiled with the catheter. In the case I am about to relate I had in vain endeavoured to introduce different instruments, and, knowing how highly M. Cazenave had extolled the merits of ice in similar instances, I determined to give his recommendation a fair trial; with what result will be seen.

On October 18th, J. B., a labourer, aged forty-six, came under my care at St. Peter's Hospital as out-patient, for an impassable stricture of long continuance, situated five and three-quarter inches from the meatus externus. Immediately in front of the stricture, in the median line, there was a false passage into which a catheter could be passed up to the hilt without drawing a drop of blood; thus demonstrating that it had acquired the respectability of age. On October 18th, and again on the 20th, I tried in vain to introduce different instruments; and on the 22nd the patient came to the hospital with an attack of retention of urine. Mr. Walter Coulson, who happened to be in the institution, endeavoured, without success, to pass a catheter, and I was sent for to take charge of my case. On my

arrival I at once employed the catheter, but without any result; and therefore resorted to M. Cazenave's plan, and plugged the rectum with small pieces of ice. Exactly twenty minutes after the last fragment had been inserted into the bowel the patient began to pass water guttatim, and in the course of half an hour contrived to empty his bladder. He was kept quiet and warm in bed, and his bowels were gently moved every day. I endeavoured every other day, for about five minutes, to pass an instrument, but without success; and on Nov. 11th I had his perineum freely leeched, and continued my attempts at catheterism, with no better result till Nov. 24th, when I fancied I had succeeded in introducing a very slender whalebone bougie into the bladder. In order to demonstrate the accuracy of my supposition, and to utilise its realisation, if correct, I attempted to slide one of my smallest grooved silver catheters over the bougie into the bladder; but in doing this I used an amount of force which nothing but the intractable and serious nature of the complaint could justify, and the result was that the bougie was cut in two about its centre, six inches of the whalebone being left behind in the stricture. As the patient experienced no pain from its presence, I determined to leave it in the urethra, in the hope that it would set up a profuse discharge, and so facilitate the treatment. In this expectation I was, however, disappointed; and on Dec. 1st I removed the broken portion of the bougie after seven days' imprisonment, the patient being all the while entirely ignorant of its whereabouts, and complaining of nothing beyond an occasional smarting when passing water. I now directed the patient to pass down a large bougie as far as it would go, and to keep it pressing against the face of the stricture for half an hour every day. Twice a week I tested the urethra with instruments to see if any progress were made, but the results were always of a negative character. The man's stricture possessed but little sensibility, and hence the unremitting assaults made on it were productive of neither pain nor annoyance.

On Dec. 19th I came to the conclusion that a fair and unusually patient trial had been afforded for the success of all ordinary means, and nothing apparently remained but the knife to cut the Gordian knot. As, however, the patient's perineum was intact, I was loth to cut into it, and the presence of a false passage did not render the most difficult operation in surgery more easy of performance. On reflection, I considered that this was a fit and proper case on which to try the "bougie armée," and I communicated my intention to Mr. Henry Smith, the redoubtable champion of this potent machine, who was kind enough to intimate to me his gladness to be present when I used it. Accordingly, on Dec. 22nd, in the presence of Mr.

Smith and two other gentlemen, I passed down to the stricture a wax bougie slightly curved, and armed with a small piece of caustic potash. For a couple of minutes I kept the bougie firmly pressed against the stricture, slightly rotating it whilst doing so; and on withdrawing it, I found the potash had all deliquesced against the stricture. Being aware of the exact relations of the false passage, I was particular in keeping the bougie's point pressed slightly upwards, in order to avoid entering the mouth of the abnormal route in the lower wall. The application of the caustic was not attended with any pain or bleeding, and on December 25th I reapplied it, the patient complaining that the orifice was very sore, as its lips had tasted the caustic on its entry to the urethra. The following day the man stated that his stream of water had much improved, but there was a gonorrhœal discharge, with, however, diminished frequency of micturition. On Dec. 27th, I again employed the "bougie armée" as before; and on the 31st, knowing the instrument had penetrated about a quarter of an inch deeper than on the first occasion, I determined to endeavour to pass a very small olivary bougie. Accordingly, before using the bougie, I gave it the well-known *coudée* turn of M. Auguste Mercier, and I had the satisfaction of seeing it enter the bladder. I then successively introduced two larger bougies, and finally left a No. 6 olivary catheter in the bladder for the night, the patient removing it at 9 A.M. on New Year's Day. The effect of leaving in the instrument was that I was enabled on the day mentioned to pass Nos. 7, 8, 9 one after the other. On Jan. 3rd I introduced No. 12, in the presence of Dr. Reyher, of Dörpat, and others; and on Jan. 5th I passed a No. 13 olivary elastic catheter, when Dr. Budgett was present, and drew off the patient's urine. The following day the patient went out on leave, returned intoxicated, and was discharged from the institution. The patient reappeared on January 31st, when I passed No. 15 olivary catheter with ease.

It will thus be seen that the satisfactory result obtained in this case was entirely due to the employment of the "bougie armée;" for rather more than a two months' trial had been given to permit the effects of rest, combined with leeching and diligent attempts at catheterism, to produce the desired result, without success. It would appear, therefore, that the application of caustic is justifiable and useful in those cases of stricture which are proved to be impassable; and it is to be used, not to tunnel through the stricture, but to open up its mouth, the impassability of the stricture being due to the excentricity of the entrance to the contracted portion of the canal. The retention which took place in the early treatment

of the case was successfully relieved by the introduction of ice into the rectum—a method which I believe is practically unknown in this country. One great advantage of the plan is that it in no way interferes with an attempt at catheterism. In the instance I have related I found the introduction of pieces of ice into the rectum gave rise to some local annoyance, which could be entirely prevented on another occasion by the employment of a speculum open at both ends.—*Lancet, Feb.* 7, 1874, *p.* 194.

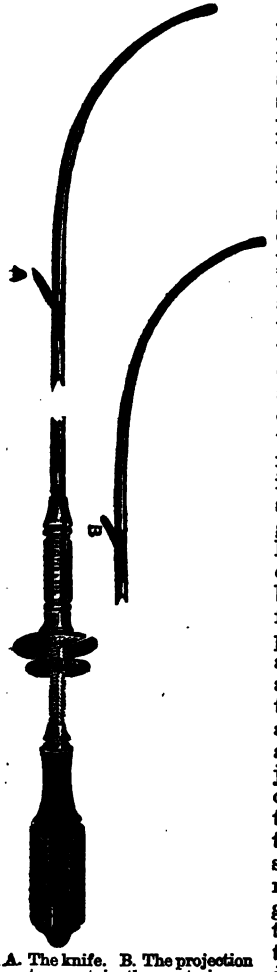
81.—AN URETHROTOME FOR INCISING NARROW STRICTURES.

By Berkeley Hill, Esq., Surgeon to University College Hospital, and for Out-patients to the Lock Hospital.

Hitherto, among the variety of instruments that have been constructed for internal urethrotomy, none, so far as I know, can be used for strictures through which a No. 6, or, at most, a No. 4 catheter cannot be passed. The treatment of such cases, whatever method of dilation be selected, does not usually present much difficulty; but, with smaller passages, the difficulty of introduction and the amount of suffering to be contended with have induced me to seek for some means by which a narrow stricture, when an instrument has been passed through it, may be at once enlarged to the normal capacity. This may be done by *internal incision* or by *divulsion;* which of the two proceedings should be adopted in any given case, depends upon certain points that are now pretty well agreed upon.

The instruments which I have here are both manufactured and, to a great extent, devised by Mr. Coxeter, who has exerted much skill in constructing instruments to carry out my suggestions—one for cutting, and the other for breaking through, the stricture. Before introducing the instrument for enlarging the stricture, the distance of the contraction from the meatus urinarius should be ascertained by passing an olive-headed bougie with a slender graduated stem. This being done, both instruments are passed in the same manner. A guide bougie of fine whalebone, about one-fourth the calibre of a No. 1 catheter, is passed through the stricture to the bladder; should the operator be in doubt if he have reached the bladder, a fine silver catheter, having its end tunneled with an eye, is threaded, and, thus guided, the catheter is slipped along the bougie and through the stricture till the bladder is reached. Urine then runs through the catheter, and makes it certain that the instruments are in the right track, when it may be withdrawn, leaving the bougie *in situ*.

However, in most cases, this precautionary exploration may be omitted.



A. The knife. B. The projection to ascertain the posterior limit of the stricture.

The next step is to pass the enlarging instrument along the guide in the same way. The incisor consists of a slender grooved sound of steel, with the ordinary curve, and provided with a tunneled end. In the groove runs this little rod having a blunt end fixed to it by a joint. When run along the groove of the sound, the jointed end and rod are quite concealed until they reach a point 6¼ inches down the shaft. Here the groove becomes shallow and ceases; thus, the jointed end is thrown out and projects about one-tenth of an inch beyond the surface of the sound. (See B in the figure annexed.) It thus forms a little catch on the sound, which will travel along the urethra until it reaches the stricture, against which it will be caught. After the grooved sound has been passed along the guide, through the stricture, the little rod may be run down, and its end made to project into the urethra behind the stricture. By drawing forwards the sound thus armed, the posterior limit of the stricture is ascertained; the anterior limit is already known. The little rod is then withdrawn and replaced by another which carries, instead of a a blunt point, a knife attached by a joint in the same way; this knife, concealed in the groove of the sound, travels down until it has reached the place where the groove grows shallow. (See A in the figure annexed.) This termination of he groove being already carried beyond the posterior limit of the stricture, the knife is thus made to project to a distance regulated by a screw at the handle against the floor of the

urethra; and, if the sound be drawn forwards, the knife cuts its way until the whole stricture is divided. When the anterior limit of the stricture is reached, the knife is again brought within its groove, and the sound is withdrawn. The knife projects half an inch as a maximum, but, by adjusting the screw, can be set to any less distance. The sound, with the knife lying in its groove, is of less calibre than a No. 2 catheter, being between that and No. 1 of Weiss's catheter scale. After the incisor is withdrawn, to make assurance more certain that the stricture is thoroughly divided, the split sound of the wedge-dilator may be passed, and the wedge which corresponds to the natural capacity of the urethra No. 14 or 16 may be run through the divided stricture.

This wedge-dilator, which I had the pleasure of showing to the profession at the Plymouth meeting, acts on the same principle as that so well known and so successful in the hands of Mr. Barnard Holt. It differs in being of simpler construction, being a wedge at the end of a rod, which, being pushed between the blades of a split sound, separates them to the required extent, and thus splits the stricture it passes through.

My dilator can be made as small as No. 1½ catheter, is easily passed along the canal, and, when through the stricture, the wedge requires very little force to separate the blades; for the surfaces of friction are together little more than half an inch in extent, instead of being the whole distance traversed by the instrument both within and without the separating tube, as in Mr. Holt's dilator.

At the beginning of the paper, I said that, when forcible enlargement is selected, the rules that influence the adoption of a cutting or a splitting instrument are tolerably well known. I may, perhaps, be allowed to recapitulate the most important.

1. All contractions in front of the bulb—that is, in the penile part—should be cut, not split, because cutting causes less cicatricial tissue to be formed than does splitting; this is important where the layer of erectile tissue is thin and easily replaced by fibrous tissue, by which erection at this point may be wholly lost.

2. Whenever the contraction is very elastic, it stretches rather than breaks before the wedge, and is best divided by a sharp edge.

3. Whenever the induration matter extends more than half an inch along the passage, or is very thick, it is usually best to make an incision through it.

In other cases, as divulsion is simpler and needs less precaution, it is on those accounts generally preferable to incision.—*British Medical Journal, Nov.* 29, 1873, *p.* 627.

82.—ON THE VALUE OF THE "BOUGIE CONDUCTRICE" IN THE TREATMENT OF STRICTURES OF THE URETHRA.

By Rushton Parker, Esq., Surgeon to the Stanley Hospital, Liverpool.

A few weeks ago Mr. Savory made some remarks upon the general clinical aspect of urethral stricture, and his allusion to the superaddition of spasm is a point as undeniable as it is liable to be overlooked.

With regard to the extensive employment of the recumbent posture, as a substitute for operative measures, I have no experience, and see little prospect of ever having it; for I am sure that in the majority of instances, especially those occurring in private practice, this cannot be even attempted. There are many men suffering from stricture, even of narrow dimensions, who find the malady inconvenient enough to wish it away, and who will gladly submit to anything that shall not confine them to the house for more than a week or so, but who would go on fighting with the stricture, unaided by surgery, to the detriment of bladder and kidneys perhaps, rather than suffer prolonged recumbency. They have not the leisure for it, to say nothing of strong disinclination.

It becomes, then, advisable to quickly dilate the urethra if possible, and the results of practice are eminently satisfactory, provided an occasional catheter be subsequently passed.

The instruments employed for divulsion have been brought to a high state of development both in simplicity and mechanical efficiency, and the dilator of Mr. Berkeley Hill is, in my opinion, the simplest and the best of these.

As to urethrotomes, those commonly used in England are not fine enough for immediate passage through a stricture less than the size of a No. 4 catheter—the urethra must be previously prepared by soaking in a fine catheter or bougie for a day,—so Mr. Hill has had constructed an ingenious urethrotome, which can, it seems, be employed in the tightest cases.

I wish to draw attention to the urethrotome of M. Maisonneuve, which has been in use, I am told, about eleven years, which is much simpler than that of Mr. Hill, which I find remarkably efficient and easy to work, adapted for the narrowest strictures, and which I think far the best urethrotome I have ever seen or heard of. It costs 16 francs.

The sound is of steel, hollow, split along the upper surface, and the size of a No. 2 catheter. The knife, of which I have two sizes, attached to a thin flexible arm of steel, which bends as it slides along the concavity of the sound, cuts in advancing and receding, but only along its slopes, not at the free apex,

which is knobbed and very blunt, blunt enough to leave uninjured the healthy part of the urethra. The lash-like appendage is a filiform bougie, fitted with a female screw at one end for attachment to a male screw at the tip of the sound.

The bougie is first passed, its size being that of an English No. $\frac{1}{2}$ or less; then the sound is screwed on to it, and pushed *after* it, the bougie coiling up in the bladder. The knife is then run down the sound and withdrawn, the sound and bougie being pulled out next. The operation is easy, rapid, and successful, and produces neither more pain nor more bleeding than does "divulsion."

The "bougie conductrice" is used in Paris with a variety of urethrotomes and dilators; and could be adapted to Mr. Hill's dilator easily, if wished for; in any case, a small metal cap covering the screw at the tip of the sound when the bougie is not used.

The advantage of this fine appendage is that when once the filiform bougie is passed, the surgeon has only to screw to it the sound of a dilator or urethrotome, which will then be safely piloted into the bladder, and the stricture may be immediately divulsed or incised.

I have only employed Maisonneuve's urethrotome in five cases hitherto (not having long possessed the instrument); thrice with, and twice without, the bougie. All except one *required* urethrotomy, the stricture being in the penile portion; but in this one the stricture was behind the penile portion, and the split sound of Mr. Hill's dilator could not be passed, while a "bougie filiforme" went easily, and so the operation was concluded with Maisonneuve's urethrotome, as I have not had the dilator fitted for bougies.

The details of my cases are not given, nothing occurring but what is ordinary. None of the patients were in bed more than a week, and some might well have been less; while one, a ship's officer, was up in two days, and out daily after five days, feeling quite well and urinating in comfort. The strictures were very small in size and of some years' duration, and varied from one to three in number in each patient. Following Mr. Hill's practice, I never tie in a catheter after uncomplicated divulsion or internal incision, nor have I had reason to regret the omission.

In cases, then, requiring rapid dilatation, I should say, "incise *all, if you like,* with Maisonneuve's urethrotome; for *some* divulsion *may* be used with equal but not greater advantage.—*Lancet, Jan.* 31, 1874, *p.* 162.

83.—THE BOUGIE CONDUCTRICE.

By W. F. Teevan, Esq., London.

I am glad that Mr. Rushton Parker likes the urethrotome, but if it be used in the manner described by him at p. 162 of

your journal of January 31st, it will repeat some of its Parisian accidents. It is now twenty years since the instrument was brought into practice by M. Maisonneuve, and it has since then been much improved by M. Voillemier, Prof. Gauley, and others. There are three points which call for remark. In the first place Mr. Parker takes it for granted that when the "bougie conductrice" has passed down the urethra it has entered the bladder, and it is in this assumption that the danger lies. The bougie may have gone into the bladder, rectum, or false sac, and it is for the surgeon to verify the situation of the bougie before he passes the knife into the urethra. Now this can only be done by using the "sonde à vis," which is unknown in this country, or by inserting in the sound a stylet which shall fill the slit, so that when it is withdrawn urine will flow if the bougie has gone into the bladder. Secondly, Mr. Parker states that the blade cannot injure the healthy part of the urethra; but the contrary has been proved in Paris, hence M. Voillemier's modification to guard the blade. Thirdly, French surgeons are now of opinion that the blade ought to slide along the convexity of the sound, and not the concavity as described by Mr. Parker. I would recommend to him Prof. Gauley's tunneled urethrotome, which is vastly superior to the one he has illustrated.

As for "divulsion," "dilatation instantanée," "immediate treatment," or any other synonym by which the process of tearing open a man's urethra may be concealed, it has fully justified the almost universal condemnation passed upon it by Parisian surgeons. It has acquired the unenviable distinction of being the most fatal operation known for stricture, and I am aware of thirty deaths following its use by different surgeons. The operation has been abandoned or condemned by Sir William Fergusson, Mr. Thos. Bryant, Mr. Walter Coulson, Mr. O. Pemberton, Mr. H. Smith, Prof. Stokes, and others.—*Lancet, Feb.* 14, 1874, *p.* 250.

84.—A DESCRIPTION OF THE "SONDE A VIS PORTANT UN CONDUCTEUR."

By W. F. Teevan, Esq., B.A., Surgeon to the West London Hospital, &c.

This useful instrument, unknown, I believe, in this country, but used by Mr. Stokes of Dublin, and by American surgeons, was introduced into practice many years ago by M. le Docteur Ch. Phillips of Paris; and when I mention that that gentleman was sent for by M. Nélaton when he was unable to pass an instrument, and that he succeeded where the great master of surgery had failed, I think it will be deemed unnecessary for me to say much for an instrument coming from such authority. The

sonde à vis portant un conducteur is, as its name expresses, composed of two parts—a flexible bougie and an elastic catheter. The bougie tapers away at one end to a fine line, whilst its other extremity enlarges to about the size of No. 2, and is fitted with a screw, which works into a socket on the vesical end of the catheter. The ends of the bougie and catheter are screwed together like the bamboo rods used by the chimney-sweeps. The bougie is first passed into the bladder, and the catheter is screwed on to it, and then pushed slowly into the bladder, causing the whole of the bougie to enter that viscus, where its great flexibility allows it to coil up and remain quietly without the patient being aware of its presence till the catheter has effected its object of drawing off the urine. The entire instrument is then withdrawn, or the catheter can be unscrewed to leave the bougie *in situ*, to serve as a guide for the next occasion, or to dilate the stricture.

The instrument is most useful. Firstly, for those cases of enlarged prostate and tumours of the bladder where the urethra is so tortuous that no ordinary catheter can be passed. The slender bougie can be wriggled in first, and so guide on the catheter. Secondly, for cases of stricture where the difficulty arises not so much from the tightness of the stricture as the deformity of the canal (Delpech). Thirdly, for the performance of internal urethrotomy in the deeper portions of the urethra. When the *sonde à vis* has demonstrated that the bougie is in the bladder, it can be unfastened from the latter, which is then screwed on to the urethrotome to conduct it into the organ.—*British Medical Journal, Feb.* 7, 1874, *p.* 171.

85.—ON THE TREATMENT OF FISTULA IN ANO BY THE ELASTIC LIGATURE.

By William Stokes, Esq., Professor of Surgery, Royal College of Surgeons, Ireland.

As the substitution of the elastic ligature for the knife in many surgical operations, a method recommended originally by Signor Silvestri, of Vicenza, and recently adopted and strongly advocated by Professor Dittel, of Vienna, is now largely engaging the attention of the surgical profession both at home and abroad, a few particulars of a case of fistula in ano, recently operated on by me in the Richmond Surgical Hospital by this method, will probably be read with interest.

William M., æt. 48, a strong, muscular man, of irritable temper and intemperate habits, was admitted into my wards in the Richmond Hospital on December 10th, 1873, suffering from fistula in ano. Six years previously he suffered from an abscess at

the left side of the anus, which was opened while he was in the Whitworth Hospital, under treatment for an attack of delirium tremens. No fistula resulted from this opening. Two years subsequently he suffered again from an abscess in this situation, and in September, 1873, another abscess formed; which burst, and a blind external fistula resulted.

On examination shortly after his admission into hospital, I perceived the external opening of the fistula, situated at about an inch and a quarter to the left side of the anus. On introducing a director in the fistula, I found that it passed upwards and inwards to an unusual height; and also that it was incomplete. In consequence, therefore, of the great length of the fistula and the distance of the external orifice from the anus, I considered it a suitable case for treatment by the elastic ligature, as by the ordinary operation, owing to the amount of tissues which would have had to be divided by the knife, the hemorrhage would probably have been very excessive and difficult of control. This, considering the patient's habits and constitution, would have been a matter of serious consequence.

I first completed the fistula by Blandin's bistoury, and then I passed a long probe, armed with a double elastic ligature, through the fistula. Owing to its great height, I had considerable difficulty in detaching the ligature from the probe in the rectum; but this I eventually succeeded in doing, and drawing out the loop through the anus I removed the probe, thus leaving the two free ends of the ligature through the orifice of the fistula. I then put the double ligature tightly on the stretch and fastened it securely. This latter procedure was accompanied by a considerable amount of pain, which continued for about an hour subsequent to the operation. A morphine suppository then administered, however, effectually relieved the pain.

On the evening of the fourth day after the ligature was applied it came away, the patient not having lost during the entire proceeding a single drop of blood. The parts divided by the ligature united with remarkable rapidity, and recently the patient returned home in excellent health and spirits.

The impression produced in my mind by the result obtained in this case has been most favourable, the case being one which, owing to the peculiar habits and constitution of the patient and the great height of the fistula, most surgeons would have certainly hesitated or declined operating on by the ordinary method of division of the sphincter.

Although I entertain grave doubts as to the alleged immunity from the occurrence of pyæmia, which Professor Dittel claims for this method, I am confident it will be in many instances preferred by operating surgeons to cutting instruments, "when," as Professor Dittel observes, "he has to undertake

operations in cavities and canals which are out of sight, or which are so narrow that it is extremely difficult, if not impossible to use cutting instruments within them, or when he feels uncertain whether he may be able to tie the bleeding vessels (as in anal fistulæ situated high up). In other cases he will desire to avoid hemorrhage and the formation of large wounds in children or in old persons."—*Medical Press and Circular, Jan.* 7, 1874. *p.* 3.

DISEASES OF THE SKIN, ETC.

86.—ON SUSPECTED RINGWORM (SCURFY HEAD.)

By Dr. TILBURY FOX, Physician to the Department for Skin Diseases in University College Hospital.

The practitioner is very often puzzled to make a diagnosis in cases of suspected ringworm. Cases, especially in schools, are brought to him which exhibit here and there—or it may be only in one small spot on the scalp—"scurfy"-looking places, without apparently any diseased hairs, and he is asked—Is it ringworm? Without the microscope it is difficult to decide the question; and I would venture to say that under such circumstances the observer can only blame himself if he falls into error by neglecting the use of the microscope, which will readily reveal in all cases whether or no ringworm is present by the appearance presented by the scales which can be scraped away from the suspected patch. The scales will always be found to have little bits of diseased hairs entangled in them where ringworm is present, and which diseased hairs are not perhaps visible to the naked eye. The accurate diagnosis of these cases is very important where schools are concerned, and a mistake in not recognising the nature of these "scurfy spots" may lead to the silent but wholesale propagation of the disease amongst the healthy. The following case affords an illustration of what I mean:

Case.—I had been prescribing for one or two children in a certain ladies' school, at different periods during two years, for ringworm of the body and head. When the mistress thought that all ringworm had vanished from amongst her pupils, she, having taken every possible means to detect at the earliest moment the faintest trace of mischief in her pupils' heads, in order to prevent the spread of the disease in her school, sent me her little daughter, aged six years, that I might look at a tiny suspicious-looking spot on the crown of the head. This spot turned out to be ringworm, and I destroyed the disease at once by iodine paint. The next day the niece (aged thirteen)

of the schoolmistress was sent to me for examination, and I learnt that two years ago the scalp of this child was noticed to be "slightly scurfy" in one or two patches here and there over the scalp. The hair thinned out slightly, but the place was not bared of hair, nor was it red. The disease "did not look like ringworm; if it had," the aunt remarked to me, "of course advice would have been sought." The child had been treated with a "little ointment" now and then; and a medical man saw her, but did not think it ringworm. The appearance of the disease in the mistress's young child induced that lady to send the niece to me, lest the "scurfy" disease from which she had been suffering might in reality be ringworm. When I examined the head of the neice, there were one or two irregular-shaped spots, the size of a shilling or so, covered over with fine micaceous scales, not devoid of hairs. The hair looked a little thin, but not more so than is commonly seen in slight cases of seborrhœa, nor did the hairs come out too easily or break off; and on a superficial glance there was no appearance of short broken-off hairs, as in ordinary ringworm. On using a magnifying glass, however, and searching over the diseased areas, certain dark-looking portions of hair-shafts came into view, and these did not run in a natural direction, but were out of the line of the normal hairs, and they were, moreover, in some cases twisted and about three or four lines in length. They were concealed in great part by the healthy hairs. They were, perhaps, five or six in each patch of disease. They turned out to be brittle, and portions came away easily when pulled at. Under the microscope the hairs exhibited the ordinary appearance of hairs invaded by the fungus of tinea tonsurans and fungus of luxurant growth.

Remarks.—The above case illustrates a not uncommon occurrence—viz., the non-detection of the nature of slightly-developed ringworm of the scalp (tinea tonsurans). Cases of ringworm may present the same characters as those exhibited by the example under notice from the outset and during their whole course; but these characters may be assumed when the disease has become chronic and is supposed to be well, for ringworm leaves behind in many cases a surface that gives off for a while furfuraceous desquamations. The diseased patches may be small—the size of a split-pea—or the area of the disease may be larger. In either case there is apparently a little scurfiness, and the hair is somewhat thinned, and that is all, save an occasional suppurating hair-follicle in the centre of the scurfy spot. But if the scales be scraped away, here and there is a bit of opaque-looking hair may be seen attached to or projecting from them, and these bits of hair will be found to be crammed full of spores. Further, in all these cases, here

and there a dark stub or two, or one or more broken-off hairs, will be detected over the scurfy surface, and afford a certain indication that the disease is parasitic. Very often, as before observed, the condition referred to occurs in a case of ringworm apparently well, and the solitary or few diseased hairs constitute so many spore manufactories to spread the disease if no parasiticide remedies are used.

The treatment of these cases consists in very carefully getting away every particle of scaliness, and fully epilating the scurfy area, and applying any simple parasiticide until the hair grows healthily again; epilation being repeated to get rid of all short, dull, and opaque-looking hairs.

Practical conclusion.—Whenever a child is brought to the practitioner for his advice on account of the presence of what seem to be scurfy-looking places on the head, if these are small, and the general surface of the scalp is healthy, they are to be inspected for ringworm. A careful search should be made for broken-off hairs, and these or the scales, and any attached hairs, should be submitted to microscopic examination for fungus elements in them. In cases of chronic ringworm, all merely scurfy patches should be carefully examined, for a solitary piece of dead hair lodged in the follicle may explain the mischief, as it is generally loaded with fungus elements, which are readily sown broadcast to relight up the old mischief if parasiticide treatment is abandoned. Such ill-developed cases of ringworm, as before observed, may be the source of infection to many a child in public institutions and schools.

Note.—Dr. Duckworth has recently called attention to the effect of chloroform in rendering diseased hairs in ringworm opaque; but it will be evident that this effect will not be marked where only two or three short hairs are present, whilst the test will be of no value where there are only diseased pieces of hair filling up the follicles and not projecting above the level of the latter.—*Lancet, Nov.* 22, 1873, *p.* 733.

87.—ON A NEW METHOD OF DETERMINING THE PRESENCE OF AND RECOVERY FROM TRUE RING-WORM.

By Dr. DYCE DUCKWORTH.

A few drops of chloroform are to be poured upon the head of the patient, who must be placed in a good light between the operator and the window. On evaporation of the chloroform, the hairs affected by ring-worm are seen to become of a yellowish-white colour, opaque, and like fine filaments of a vegetable lichen. This change is observable, not only upon the hairs, but

also on the skin in the immediate neighbourhood. Small whitish masses are seen upon the scalp, and especially at the point of emergence of the hairs. The healthy hairs are quite uninfluenced.

[Dr. Robert Liveing makes the following remark upon this.] Having tried Dr. Duckworth's method, I can bear testimony to the fact that the effect produced on patches of ring-worm by chloroform is striking, and that healthy skin and hair are not affected by it. It appears to me, however, that a similar appearance is produced, though in a less degree, on scaly patches of skin unaffected by parasitic growth; in other words, that the appearance does not depend upon the presence of the parasite, but on an altered condition of the cuticle and hair which may or may not result from it.—*Lond. Med. Record, Dec.* 3, 1873, *p.* 766.

88.—THE TREATMENT OF NON-PARASITIC SYCOSIS.

By Dr. TILBURY FOX, University College Hospital, London.

No disease, I take it, is more unsatisfactory to treat than the common inflammation of the hair-follicles of the beard and whiskers, to which the term sycosis non-parasitica is applied. On the continent, especially in Germany, the practitioner is advised to adopt epilation, and to apply some simple astringent ointment; and there is a great disposition nowadays to regard epilation as *the* remedy for the disease under notice. The reasons for epilating is variously stated. Some affirm that the inflammation in sycosis is caused by a premature development of a new hair in the follicle, and that it is necessary to its cure to rid the follicle of the old hair. Others think that suppuration extends to the root of the hair, and that epilation relieves the tension of the parts and permits the exit of the pus. The first explanation will not bear examination. The second is true in part. In non-parasitic sycosis inflammation travels downwards, and may reach the bottom of the follicle, the root of the hair being bathed in pus, whilst the hair is loosened from its surrounding connexions, and lies, as it were, a dead piece of tissue in the follicle. In such cases epilation does but get rid of the loosened hair, and its extraction allows the escape of pus that would otherwise be pent up. But in many cases the inflammation does not proceed to the extent of causing suppuration in the deep part of the follicle, the hairs are not loosened in the follicles, and their extraction gives great pain, and can do no good. Epilation is, therefore, a fit procedure only at a certain stage of sycosis—if the skin is much inflamed, the follicles freely suppurating, and the hairs are thereby loosening or loosened in them.

The treatment which I have found most successful may be summed up as follows :—In the early stage, when the follicles are very hyperæmic, saline aperients in persons of full habit; or aperient tonics, such as sulphate of magnesia with sulphate of iron, in those who are debilitated; together with hot fomentations, and simple, soothing applications which exclude the air, locally. When there is free suppuration, the same internal remedies, together with the removal, by epilation, of the loosened hairs from freely suppurating follicles, and the application of mild astringents, such as zinc lotions and ointment; and, lastly, in the sub-acute or chronic stage, where there is only a suppurating follicle here and there, but mostly a number of indurated tubercles—i.e., follicles thickened by hyperplasic growth of the connective tissue,—a course of Donovan's solution, together with, locally, hot fomentation and the application of a weak nitrate-of-mercury ointment (a drachm and a half to an ounce) night and morning. Of course, for persons of scrofulous constitutions, cod-liver oil and iron are to be given in combination with alterative remedies. I fully admit that the exhibition of Donovan's solution is in great part an empirical proceeding; but I prefer it to any other remedy, and have reason to speak with confidence as to its efficacy in sycosis when employed in the way and at the particular stage above indicated. Lastly, I may add that it is an easy matter to do harm in sycosis by the injudicious use of local irritants, which intensify the hyperæmia and the hyperplasic thickening; and I believe this to be the radical fault in the treatment of sycosis. —*Lancet, Dec.* 27, 1873, *p.* 902.

89.—HYPODERMIC INJECTION OF CARBOLIC ACID IN ERYSIPELAS.

Dr. AUFRECHT, of Magdeburg, having last year lost four patients of advanced age who were attacked by erysipelas of the extremities after injury, determined to try the effect of carbolic acid, and in a short paper in the *Centralblatt*, for February 21, he communicates the results which he obtained in two cases. If (he observes) it be true that erysipelas in such cases as these arises from the penetration of organisms into the subcutaneous tissue, and their multiplication there, and if carbolic acid possess the power of destroying such organisms or of impeding their injurious influence, this substance should be able to prevent the spreading of the erysipelas, and to a certain extent diminish its danger. In order to ascertain whether carbolic acid may be hypodermically employed without any ill consequence, he experimented upon himself with a 1 per cent. solution, of which he threw in six decigrammes at a time

—*i.e.*, the amount contained in an ordinary Pravaz's syringe. Neither local nor general ill-effect resulted. Since then he has employed the injection in two cases—the one a woman aged fifty-six, with erysipelas of the forearm and the hand, arising from a slight abrasion; and the other a man, aged eighty-two, with erysipelas of the thigh following slight ulceration of a cicatrix. In the first case five injections were employed during three successive days, and in the second four injections within two days. The injections were thrown into the sound subcutaneous tissue just beyond the margin of the erysipelas as it advanced towards the trunk. Its progress was at once arrested in the direction where the injections were made, the injection being repeated in consequence of some insular erysipelas appearing beyond the first injection-points. More remarkable still than this limitation of the erysipelas was the decided influence of the injections in diminishing the febrile action and the frequency of the pulse, and in inducing a general improvement in the patients' condition. Convalescence was quite satisfactory in both patients.—*Medical Times and Gazette, March* 28, 1874, *p.* 355.

90.—ON THE LOCAL TREATMENT OF CARBUNCLE.

By Dr. PETER EADE, Physician to the Norfolk and Norwich Hospital.

[Carbuncle occurring on the face is a most serious affection, and as Dr. Eade has lately had a case in which he succeeded in cutting short its progress, he records in the following paper the treatment adopted.]

In a short communication published in the Lancet for Dec. 11th, 1869, I recorded the particulars of a case of extensive carbuncle of the back of the neck which I had treated by the free application of carbolic acid to the diseased part, and especially by its free insertion into the holes and sinuses which had formed, as is usually the case, over the central portion very early in the course of the disease. An experience since that time of two or three cases of large carbuncle and of many cases of carbuncular boils, treated by the same method, has proved to me that the action of this remedy is so definite and so constantly beneficial that I have no hesitation in recommending it to the favourable notice of the profession.

My recent case was this:—A gentleman of seventy years of age, but still strong and hale, had suffered for some weeks from asthmatic bronchitis, from which he had in great measure recovered, when he was suddenly attacked (now some five weeks ago) with pain and hard nodular swelling of the lower lip near

to its right extremity. Almost immediately a double festering pimple formed over the centre of this swelling, and the painful induration rapidly extended past the corner of the mouth to the adjacent cheek, and, to a less degree, along the substance of the lip, causing also considerable œdematous swelling of the whole of this part. On the second day severe constitutional symptoms manifested themselves, and feelings of extreme illness were complained of. During this day and the next the tumefaction and hardness continued to increase, the festers over the centre of the original spot opened and were moist with dirty pus, the whole of the lower lip and right corner of the mouth and the adjacent cheek became tense and swollen, and it was quite evident that a carbuncle was forming on the right side of the face. Into the centre of the two holes which had formed I now pressed with a probe some threads of lint soaked in a strong solution of carbolic acid in oil (one part to four), and I also laid a piece of lint wet with the same over their apertures, so as to supplement the small quantity which the shallow sinuses would contain. A little smarting was complained of, but the application was repeated after a few hours, and again the following day. Almost at the end of twenty-four hours it could be perceived that a check had taken place in the morbid process, but by the next day it was plainly evident that the inflammation and induration were really beginning to subside. The carbolised lint was still carefully and scrupulously thrust to the very bottom of the small holes, and from this time no further spread of the disease took place, but, on the contrary, there was a rapid subsidence of the œdema, and in two or three days more little remained but some diffused swelling of the lower lip, some tender induration at and around the seat of the original pimple, and the ragged discharging opening which had formed at the site of the primary festers. The disease was therefore stayed, and in a few days more the patient was convalescent. He is now quite recovered.

I should scarcely have ventured to instance this case as proving the aborting power of carbolic acid over carbuncular or furuncular inflammation—seeing that the disease never attained to very formidable dimensions, and that possibly it might not under any circumstances have increased to a very serious size or extent,—had I not witnessed precisely the same effects in other cases as were here produced by the application of the acid. My conviction, nevertheless, is that the carbuncle was not only rapidly extending, but that it would quickly have become both large and important. And I cannot help contrasting its course with that of one which I had the opportunity of seeing, in its later stages, some three years ago, where the disease attacked the same parts of the face, where only the usual treatment by

poultices &c. was adopted, and where (instead of a duration of about one week for the severe symptoms, and of about ten days more for recrudescence) the suffering patient passed through a morbid process which took many weeks to complete its course, and which not only caused great and prolonged suffering, with great temporary disfigurement, but even at one time appeared to threaten life itself.

As I have stated, I have now used carbolic acid in this way in several cases of carbuncle; and in all of them its application has been followed by a uniform and immediate check to its increase, and a speedy amelioration of the local conditions. When it has been applied early, it has plainly gone far to abort the disease; and when it has been commenced later, wherever it could be brought into contact with the inflamed and hardened tissue, there at least no further spreading has taken place, whilst swelling and tension have diminished, and dirty suppurating slough has quickly given place to florid healthy granulation. And, from my observation of its action, I entertain no doubt that, if it could be brought sufficiently early into contact with the spreading disease, it would be quite competent to prevent its extension beyond the degree to which it had already advanced. Unfortunately, the acid appears to have little or no influence when applied over the unbroken skin; but directly it can be brought into contact with the diseased mass, either by being inserted into the sieve-like holes, or by being applied to it after being laid open by incision, its beneficial action becomes at once manifest.

Much credit is, I think, due to the late Mr. Startin for having suggested that both boils and carbuncles might be due to the growth and spread in and beneath the skin of a parasitic development; and the efficacy of the treatment he was in the habit of adopting, the remarkable effect of carbolic acid in checking its increase, and the almost certainty with which boils in their early stage may be aborted by applying freely to the core of the festering pimple the acid nitrate of mercury, go far to prove the truth of this opinion.

It is extremely unfortunate that the carbolic acid cannot always be brought into sufficient contact with carbuncles in their early stage—partly because of the insufficiency of the sieve-like openings, and partly because the parasite-studded (?) growth may have already extended beyond the central portion over which these are situated. But even with a very imperfect application I have observed a distinct retardation of the circumferential spread when the acid has been carefully introduced into these holes, conveying to my mind the impression that some destructive influence had been exerted upon the central root or stem of the diseased mass; and in all cases which I have

hitherto observed, no extension of the disease has taken place from any part to which the acid has been fairly applied. Hitherto I have only applied it through the natural openings, or after the mass has been exposed by incision or caustic; but I think it extremely probable that a beneficial effect would be produced by the subcutaneous injection of a watery solution of the acid around its edges (after the manner in which it has lately been employed in the case of spreading erysipelas and some other diseases), or possibly the spread of a large carbuncle might be stayed by drawing through it some small loop-holed drainage tubes, and allowing the carbolised fluid freely to percolate through these.

The strength of the solution of carbolic acid which I have employed has been about one part of the acid to four or five of the solvent (oil or glycerine), and its efficacy, I would repeat, has appeared to be limited almost absolutely to those parts with which it could be brought into actual contact; and although it appears occasionally to have produced injurious effects when used in large quantity, yet I have kept a large sloughing and granulating surface for days together constantly covered with the carbolised oil, without any harm arising, although the urine soon presented the peculiar blackish colour which has been several times observed during its employment.

I will only further add that, in looking over the various methods of treatment which have from time to time found favour with the profession, there seems to have been a constant undercurrent of feeling in favour of such remedies as have a more or less caustic or antiseptic (? parasiticide) influence. Terebinthinates, resinous applications, caustic potash, and several other caustics have been held by various competent authorities in high esteem, and their partial usefulness would seem to fortify the theory of the vital, the living nature of the disease—a theory which I think is greatly supported by the decided influence upon it of carbolic acid, a substance so well-known to possess a special power of preventing the increase and development of low forms of life and cell-growth.—*Lancet, March* 28, 1874, *p.* 439.

SYPHILITIC AFFECTIONS.

91.—WHEN AND HOW TO USE MERCURY IN SYPHILIS.

By JONATHAN HUTCHINSON, Esq., Senior Surgeon to the North London Hospital, to the Royal London Ophthalmic Hospital, and to the Hospital for Skin Diseases, Blackfriars.

As regards the precise method of administration of mercury, I confess that it does not seem to me a matter of much impor-

tance, provided it be done cautiously enough. If by the mouth, the doses must be small—at any rate at first, until the degree of the patient's tolerance has been estimated. I have seen the hypodermic injection plan pursued in Paris and Berlin, but nothing which came under my observation induced me to think favourably of it. The inunction method is safe and very efficient, but it is dirty and inconvenient, and probably nothing can be said in its favour which may not be alleged with greater force of the vapour bath. This latter method, as introduced by Mr. Langston Parker and perfected by Mr. Henry Lee, is an admirable plan. Its only drawbacks are the expense and trouble which attend it. At Aix-la-Chapelle, which may, I suppose, in some sense be regarded as a better-class Lock Hospital for the north of Europe, amongst medical men of immense experience in the management of constitutional syphilis, the inunction plan still enjoys undisputed pre-eminence. From my friends, Dr. Diemer, Dr. Wetzlar, Dr. Brandis, and Dr. Ziemssen, of that place, I heard but one report: all of them believed in mercury as the one cure for syphilis, and inunction as the best method of using it.

In order that I may not fail in clearness I will, at some risk of being accused of dogmatism, put my conclusions in a categorical shape. They are as follows:—

That mercury is probably a true vital antidote against the syphilitic virus, and that it is capable of bringing about a real cure.

That, in practice, a good many cases are really cured by mercury; the cure being proved by the restoration to good health; and, in some cases, by renewed susceptibility to contagion.

That the probability of cure depends upon the stage of development attained by the disease when the remedy is resorted to, and upon the perseverance with which it is used.

That, in order to secure the antidotal efficacy of mercury against syphilis, it is desirable to introduce a considerable quantity into the system, and to protract its use over a very long time.

That ptyalism and other evidences of the physiological action of mercury, so far from being beneficial, are, if possible, to be carefully avoided, since they prevent the sufficiently prolonged use of the remedy.

That in cases in which the patient shows an idiosyncrasy peculiarly susceptible to mercury, the indication is to reduce the dose rather than to omit the drug.

That it is impossible to begin the administration of mercury too soon, and that it should be resorted to without loss of time in all cases in which a chancre shows a tendency to indurate.

That many cases of indurated chancre, treated early by mercury, never show any of the characteristic symptoms of the secondary stage.

That in other cases of mercurial cure of the chancre, in which yet secondary symptoms do occur, they are usually milder than if allowed to develop without specific treatment.

That, when mercury does not wholly abrogate the secondary stage, it exhibits a remarkable power in delaying it.

That delayed outbreaks of secondary syphilis are to be regarded rather as proof that the administration had not been sufficiently persevering than that the remedy was not efficient.

That it is probable that the risk of tertiary symptoms is in ratio with the severity and prolonged duration of the secondary stage.

That there are some grounds for believing that the tertiary symptoms of syphilis are both less frequent and less severe in those who have been efficiently treated by mercury than in others.

That mercury, cautiously given, does not, in a great majority of instances, do any injury to the general health, and that its local inconveniences may usually be prevented.

That the doctrine of the real antidotal character of mercury, in respect to syphilis, ought to lead to much more prolonged administration of it, with the hope of destroying utterly all lingering germs of the malady.

That most collected statistics as to the duration of treatment and freedom from relapse are misleading and worse than useless, because usually the treatment was far too short to be effectual.

That it has not yet been proved that there are any special forms of syphilitic disease in which mercury ought to be avoided, although, as a general rule, it is acknowledged that it must be used with more caution in all forms which are attended by ulceration than in others.

That iodide of potassium possesses little or no efficacy against either the primary or secondary form of syphilis.

That the efficacy of mercury is often most signally proved in cases which have utterly resisted the action of iodide of potassium.

That it does not much matter whether mercury is given by the mouth, by inunction, or by the vapour bath, provided that, whichever method be selected, care be taken to avoid salivation, purging, &c.

That the doses usually resorted to for internal administration are for the most part too large, and thus often necessitate a premature discontinuance of the remedy.

That if one method of administration does not proceed satisfactorily, another should be tried; and that in no case of difficulty should the vapour bath be forgotten.—*Lancet, Jan.* 31, 1874, *p.* 159.

92.—MERCURY IN SYPHILIS.

By the EDITOR OF THE MEDICAL TIMES AND GAZETTE.

The paper recently read before the Hunterian Society by Mr. Hutchinson, on the giving of mercury in syphilis, is one which merits the most careful attention of the profession, and this, too, all the more that it contains a kind of retractation of former opinions more opposed to mercury than those now entertained by the author. Unfortunately, the constant dinning of the evils of mercury into the ears of the public has not been without effect, and a kind of dread of its evils has arisen in men's minds, which experience does not justify. Not every venereal sore is syphilitic; not every syphilitic sore is venereal. The former proposition gives us the clue to the bias against mercury, for it was used in olden times not only against all kind of venereal sores, but even against gonorrhœa. The latter commends itself mainly to medical men who run risks of infection to which the ordinary public are not liable, and to whom, therefore, personally the proper cure of syphilis is of the first importance. Again, in dealing with syphilis, men too much lose sight of the fact that there are slight cases of syphilis, just as there are severe cases—that certain cases tend to get well, just as certain cases tend to go from bad to worse; but, as pointed out by Mr. Hutchinson, even the worst cases may be benefited by mercury if judiciously given. In the giving of mercury, blue-pill has had too much place. As a preparation it is bulky, and it tends when long given to upset the stomach. Syphilis is really (as shown by Mr. Hutchinson) a kind of fever, lasting, however, much longer than do ordinary fevers—sometimes even a lifetime. If, therefore, we are to benefit our patients, we must be content to give mercury in such a way as will least interfere with nutrition—to give it, therefore, in small doses, and over a lengthened period of time. In this way perhaps the liquor hydrargyri perchloridi, in drachm doses, is one of the best modes of exhibition, for this contains only a very small quantity of mercury, and, given in a vegetable infusion, is often of the utmost benefit. But in many cases there can be no doubt whatever that inunction, or the vapour-bath, suits better than any other mode of giving the remedy; and a paraphrase of the old rule—viz., the stomach for food, and the skin for mercury—might be held to be the best mode of solving the difficulty.

As to the good effects of mercury, if skilfully given, there is nowadays such a consensus of opinion that no wise man would attempt to dispute it any more than he would dispute the value of iodide of potassium in the later stages of the malady. But the word "skilfully" implies much: it means sometimes giving large doses of one form of the remedy, sometimes small doses of another. That mercury is truly curative in many cases of syphilis, we firmly believe; nor, on the whole, are we inclined to quarrel with the following categorical conclusions by Mr. Hutchinson, namely:—

"That mercury is probably a true vital antidote against the syphilitic virus, and that it is capable of bringing about a real cure. That, in practice, a good many cases are really cured by mercury, the cure being proved by the restoration to good health, and in some cases by renewed susceptibility to contagion. That the probability of cure depends upon the stage of development attained by the disease when the remedy is resorted to, and the perseverance with which it is used. That, in order to secure the antidotal efficacy of mercury against syphilis, it is desirable to introduce a considerable quantity into the system, and to protract its use over a very long time. That ptyalism and other evidences of the physiological action of mercury, so far from being beneficial, are, if possible, to be carefully avoided, since they prevent the sufficiently prolonged use of the remedy. That in cases in which the patient shows an idiosyncrasy peculiarly susceptible to mercury, the indication is to reduce the dose rather than omit the drug. That it is impossible to begin the administration of mercury too soon, and that it should be resorted to without loss of time in all cases in which a chancre shows a tendency to indurate. That many cases of indurated chancre treated early by mercury never show any of the characteristic symptoms of the secondary stage. That in other cases of mercurial cure of the chancre in which yet secondary symptoms do occur, they are usually milder than if allowed to develope without specific treatment. That when mercury does not wholly abrogate the secondary stage, it possesses a remarkable power in delaying it. That delayed outbreaks of secondary syphilis are to be regarded rather as proof that the administration had not been sufficiently persevering than that the remedy was not efficient. That it is probable that the risk of tertiary symptoms is in ratio with the severity and prolonged duration of the secondary stage. That there are some grounds for believing that the tertiary symptoms of syphilis are both less frequent and less severe in those who have been efficiently treated by mercury than in others. That mercury cautiously given does not, in a great majority of instances, do any injury to the general health, and

that its local inconveniences may usually be prevented. That the doctrine of the real antidotal character of mercury in respect to syphilis ought to lead to much more prolonged administration of it, with the hope of destroying utterly all lingering germs of the malady. That most collected statistics as to duration of treatment and freedom from relapse are misleading, and worse than useless, because usually the treatment was far too short to be effectual. That it has not yet been proved that there are any special forms of syphilitic disease in which mercury ought to be avoided, although, as a general rule, it is acknowledged that it must be used with more caution in all forms which are attended by ulceration than in others. That iodide of potassium possesses little or no efficacy against either the primary or secondary forms of syphilis. That the efficacy of mercury is often most signally proved in cases which have utterly resisted the action of iodide of potassium. That it does not much matter whether mercury is given by the mouth, by inunction, or by the vapour-bath, provided that, whichever method be selected, care be taken to avoid salivation, purging, &c. That the doses usually resorted to for internal administration are for the most part too large, and thus often necessitate premature discontinuance of the remedy. That, if one method of administration does not succeed satisfactorily, another should be tried; and that in no case of difficulty should the vapour-bath be forgotten."—*Medical Times and Gazette, Jan.* 24, 1874, *p.* 99.

93.—IODIDE OF POTASSIUM IN SYPHILIS.

By the EDITOR OF THE MEDICAL TIMES AND GAZETTE.

The value of mercury in syphilis has been so amply vindicated by Mr. Hutchinson, that we may be excused from looking longer on this side the picture; but there is another which is of equal or greater interest. As well pointed out by Mr. Hutchinson, syphilis is a malady more or less approximating to the characters of an eruptive fever, and tending like these to terminate of its own accord, with the evolution of certain symptoms. What Mr. Hutchinson has laid down as regards mercury, and what in another column Dr. Wilks corroborates, is that mercury distinctly interferes with the evolutions of these symptoms, cuts the various stages short, and acts as an antidote to the specific poison which gives rise to the symptoms in question. But whether the due course of the malady be interfered with or not, it tends to limit itself and to come to a spontaneous end. But when the end has come, the individual does not cease to be liable to suffering; he ceases to be a source of danger to others, for the disease in this stage can no longer be

propagated by him—except, indeed, we accept the theory recently advanced by Mr. De Méric, that a simple sore in such a patient is capable of propagating syphilis. But he becomes liable to certain evils of no mean importance, such as periostitis, disease of bones, deposits of gummy matter in nearly all the organs of the body, rupia and spreading ulcers of the skin and other parts, to say nothing of waxy degeneration of many different organs. Now, these evils are directly consequent on syphilis, yet they are, not strictly speaking, due to syphilis; they are sequelæ, and must be treated in a totally different fashion from syphilis itself. During the period of syphilitic eruption it is questionable whether iodide of potassium is of the slightest value, whilst mercury undoubtedly exercises a most important influence on the evolution of the disease; but in this stage, which is commonly spoken of as the tertiary stage of syphilis, the value of iodide of potassium is just as unquestionable as is the value of mercury in the earlier phases of the malady. It is a very important question for us to settle—if settle it we can—What is the value of mercury in this after-stage of syphilis? Suppose we see a patient with well-marked rupia, with periostitic pains, and other signs of tertiary syphilis, who, nevertheless, has not taken mercury, what should we do? The first thing to note is that such symptoms, though usually sequelæ, may occur in the active stage of syphilis; and as long as syphilis is active, mercury will be of use; but as soon as the active symptoms have passed away, and the so-called tertiary stage begins, we must abandon the attempt to cure by mercury—we must give iodide of potassium. It is not always easy to say where the one stage ends and the other begins, but broadly speaking, tertiary syphilis or the sequelæ of syphilis may be laid down as beginning with the stage of gummy deposits; and wherever these exist, iodide of potassium, and not mercury should be given.

There is one matter of great interest with regard to the giving of iodide of potassium in such cases. Under ordinary circumstances, if we give a patient a dose of fifteen grains three times a day, we shall soon have him running at the nose and eyes, and with a rash all over his skin; but in the sequelæ of syphilis we may give twenty, thirty, or even sixty grains every four hours, and only benefit accrue. There is no rash and no other symptoms of iodism. But iodide of potassium is not the only remedy to be given in such cases. Iron and quinine are always of service, as we might almost conclude *à priori* from the pallid and anæmic look of such patients; but cod-liver oil seems often of even greater value, as it is in chronic rheumatism. But, over and above these, sarsaparilla is of undoubted efficacy. Many people think little of its effects, and are inclined to sneer

at its use. This most frequently arises from the mode in which it is given, for the decoction should be administered, not by the ounce, but by the pint; and, so given, its value is great.

It is therefore of the first importance to be able to recognise the stage in which a patient is at the time when seen. Whatever the nature of the symptoms of syphilis, if the disease is in process of evolution, mercury will do good; but if that be past, and only the sequelæ left, it will as certainly do harm. Then is the time for iodide of potassium and sarsaparilla.—*Medical Times and Gazette, Feb.* 7, 1874, *p.* 154.

94.—IODIDE OF POTASSIUM AND CARBONATE OF AMMONIA IN THE TREATMENT OF SYPHILIS, INTERNAL ANEURISM, AND CHRONIC RHEUMATISM.

By Dr. JOSEPH P. M'SWEENY, Carlow, Ireland.

Sir James Paget was the first to call the attention of the medical profession to the following interesting fact; viz., that carbonate of ammonia greatly increases the therapeutic action of iodide of potassium. I have had extensive experience in the treatment of syphilis, and have tried it with the best results, and find that five grains of iodide of potassium, combined with three grains of carbonate of ammonia, are equal to eight grains of the potassium salt administered in the ordinary way. The following case is a good example.

John ——, aged 50, consulted me about a sore situated on his left arm. There was a profuse discharge from it, and the smell was intolerable. On asking him a few questions, I got the following history. He had been a married man, his wife having died a short time ago; he had no children. Some years ago he contracted syphilis, and was treated by mercury, pushed to excessive salivation. The secondary symptoms had been well marked, and the sore about which he consulted me was of eight months' standing. He consulted several surgeons, and could get no relief. I ordered him five-grain doses of iodide of potassium, combined with three grains of carbonate of ammonia. After taking a few tablespoonfuls of the bottle, the bad smell altogether disappeared, as a man told me who was sleeping in the same room; at first he could not bear the smell, but after taking a few tablespoonfuls of the bottle he could detect no smell. This man remained under my care for about a month, and in that short time was perfectly cured, and in very good health and spirits. I could publish five cases with almost similar results. I have also found it of the greatest service in the treatment of internal aneurism, by relieving the pain and helping to consolidate the tumour. Dr. Frerichs has recommended iodide of potassium in the treatment of the waxy liver

occurring in syphilitic patients. I think that the preceding facts are well worth the notice of the profession; but I would caution medical men how they increase the dose of the iodide of potassium, for, if the carbonate of ammonia be good, it will greatly increase the therapeutic action of the iodide.—*British Medical Journal, Jan.* 10, 1874, *p.* 39.

95.—THE BROMIDE OF POTASSIUM IN THE TREATMENT OF GONORRHŒA.

By Dr. JOHN W. BLIGH, Montreal, late House-Surgeon, Kidderminster Infirmary.

[Dr. Bligh does not wish to speak too authoritatively upon this subject, as there must always be some difficulty in distinguishing the *post hoc* from the *propter hoc*, in a disease which *per se* tends towards recovery.]

Its Modus Operandi.—As the result of careful and repeated experiments, bromide of potassium is said to have the power, amongst its various other properties, of—

1. Diminishing the secreting functions of all mucous surfaces.
2. Acting as a direct sedative to all the nerves which supply the mucous membranes.
3. Increasing the flow of urine.
4. Acting as a direct sedative to the organs of generation.

It will be found to act beneficially in all the four foregoing ways. By acting as a direct sedative to the organs of generation, it prevents an increased flow of blood to the parts, thus preventing engorgement, with subsequent increased inflammation, so often followed by exudation and its train of ugly after-symptoms. Thus, through its power of diminishing the secreting functions of all mucous surfaces and acting as a direct sedative to all the nerves which supply them, it limits the gonorrhœal discharge and relieves the pain in micturition, by rendering the urethra less susceptible of irritation. In common with other remedies which augment the secretion of urine, the bromide of potassium renders this fluid less stimulating, by increasing its flow. It will thus be found serviceable throughout the whole course of the disease, but more especially in the inflammatory stage, in which so many other remedies are debarred.

This salt seems to have the power of diminishing the irritability of mucous membranes, not only when taken internally, but also when applied topically, for, according to Dr. Ringer, some writers aver "that merely brushing the pharynx and soft palate with a solution is sufficient to quell the irritability of these parts, so as to enable the laryngoscopic

examination to be made with ease." Assuming such to be the case, it may naturally be inferred that a similar effect would be produced by its topical application to the inflamed urethra. This, I believe, will be found to be the case, for which reason an injection is recommended, as will be seen when its mode of administration comes to be considered.

Complications.—It may be used with advantage in some of the complications to which gonorrhœa gives rise, especially in that distressing and frequently troublesome one, chordee. For by reason of its power as a direct sedative of the organs of generation, it subdues the sexual appetite and prevents any tendency to erection, which, as is well known—when inflammation has extended to the corpus spongiosum urethræ, and the cells are glued together by the effusion of coagulable lymph—is sure to produce this painful effect.

It will also be found useful in the third stage, when the disease has degenerated into a gleet.

Mode of Administration.—As soon as a patient complains of gonorrhœa, the bromide of potassium should be immediately commenced, and continued throughout the duration of the disease. As it is said to increase the acidity of the urine, a condition not at all desirable, some alkaline bicarbonate should be combined with it to counteract this tendency. The following formula has been found useful:—

℞. Potassæ bicarbonatis gr. 60; potassii bromidi gr. 90—120; tincturæ hyoscyami f. ʒss; aquæ camphoræ f. ʒvss. Mis. fiat mistura.

One-sixth part of this mixture to be taken three times a day, and once during the night, should the patient happen to be awake.

Care should be taken not to administer a dose whilst a meal is in process of digestion in the stomach, as it may, by neutralising the gastric juice, interfere with the conversion of the food into chyme.

If the disease is in the first stage, an injection of the salt is ordered and recommended to be used as frequently as opportunities allow. The following is the usual form and strength in which I employ it:—

℞. Potassii bromidi gr. 120; glycerini f. ʒss; aquæ distillatæ f. ʒ vss. Mis. fiat injectio.

One syringeful to be used every four hours.

When the discharge has assumed the form of a gleet, a similar injection, associated or not as may be thought advisable with some astringent, will be found useful. In addition, I am accustomed to administer during this latter stage from fifteen to twenty grain doses, three times a day, combined with fifteen minims of the tincture of the perchloride of iron, and dissolved in some suitable menstruum.

[N.B.—I see no reason why an injection of the bromide should be contra-indicated in the inflammatory stage, but, on the contrary, consider its anæsthetic properties (when applied locally) as likely to be of much service. I am, however, unable to speak from experience, never having made use of it in this stage.]

When there is any disposition to painful erections or chordee, a draught containing about half a drachm in an ounce of camphor mixture, administered at bedtime, will be found to allay this tendency almost to a certainty. In this complication its effect seems magical, and has only to be tried to be recognised as a boon of inestimable value.

There are certain accessories which should not be neglected in this, any more than in any other plan of treatment. The bowels should be carefully regulated, the proper diet prescribed, and a total abstinence from beer and other stimulants insisted on. Rest should be enjoined, and over-exertion strictly avoided. The testicles should be supported by a suspensory bandage, and the genitals bathed from time to time, especially before retiring to rest. The flow of urine may be increased by the free use of diluents, as linseed tea, barley water, &c.

My object in being thus precise in detailing its mode of administration has been to endeavour to induce others, who enjoy a far wider field for experiment than mine, to make use of the drug; when I feel confident its value as an additional agent towards the cure of a loathsome disease will be appreciated, and my views herein expressed verified. In the whole range of the Pharmacopœia there is no drug which in my opinion *primâ facie* promises such happy results in the treatment of gonorrhœa. For, as has already been pointed out, not only do we find it to have the power of diminishing the secretions and assuaging the pain of all mucous membranes, but also to produce a *special* sedative effect upon the organs of generation. Undue administration has been said to give rise to debility, but exhibited in the doses here recommended, all fear of this kind may be laid aside. Indeed, from considerable experience in its administration in large doses, continued over a lengthened period for the relief of epilepsy, I am inclined to think that its ill effects have been greatly exaggerated.—*Practitioner*, *Feb.* 7, 1874, *p.* 100.

96.—THE TREATMENT OF GONORRHŒA BY LOCAL REMEDIES ONLY.

By Dr. PHILIP FOSTER, Leeds.

Some most eminent authorities having condemned the use of injections in the first stage of gonorrhœa, I feel that I am

simply discharging a duty in recording how exceedingly successful, during an extended trial, this mode of treatment has proved in my hands. That cases do occasionally occur, as Case 2, in which the immediate use of injections would not be prudent, I willingly admit, but experience has assured me that the great majority of cases may be thus treated with the most perfect safety and success. Where mischief has followed this mode of treatment,—and I have seen instances of it, although not in my own practice,—I believe it to have arisen from injections of too powerful and irritating a nature having been used. My success in the treatment of this complaint I consider mainly attributable to the use of very mild injections; at first, say one or two grains of sulphate of zinc to the ounce of water, strengthening or changing them in a few days if necessary. And here let me distinctly state that it is not to any particular form of injection, —for one will frequently answer where another has failed,—but to the principle of treatment itself that I wish to direct attention. Three or four days of this treatment usually suffice to effect a cure, but I consider it advisable, and always recommend, that it should be persevered with for a few days after the discharge has entirely ceased. The occurrence of gleet and stricture would, I believe, be exceedingly rare after this mode of treatment.

I beg to observe that the following are not selected cases, but the only complicated ones that have come under my care since I commenced my present mode of treatment. Case 1 is inserted because it illustrates the mischievous effect of strong and irritating injections—the reason, I believe, of local treatment having fallen into disrepute.

Case 1.—Mr. —— called upon me, complaining of great pain and uneasiness in the urethra, especially when passing water. The meatus looked red and irritable, and there was a slight glairy discharge issuing from it. He had the day before consulted a surgeon at Manchester, where he resides, who had prescribed an injection of nitrate of silver, gr. j. ad ℥j., to be used three times a day. He said that this injection, which he has used twice, had made him decidedly worse. I ordered an injection of sulphate of zinc, gr. j. ad ℥j., to be used six or eight times a day. The next day he wrote to inform me that all the symptoms had entirely disappeared, and I have since heard that there has not been any return of them.

Case 2.—In this case the whole penis was exceedingly swollen, and there was great constitutional disturbance. Ordered antimony and saline aperients, sugar of lead lotion, rest, and low diet. A week of this treatment having sufficed to get rid of the swelling and fever, an injection of sulphate of zinc, gr. j. ad ℥j., was directed to be used four times a day. As, however, a week passed by and the discharge still continued, although

the zinc had been increased to gr. ij., cupri sulph gr. j. ad ℥j., was substituted for the zinc. On the fourth day after commencing with the copper the discharge had completely ceased, and the patient has remained quite well.

Case 3.—This commenced as an ordinary case of gonorrhœa. An injection of zinci sulph. gr. ij. ad ℥j., was prescribed four times a day. On the third day the patient was decidedly better, but on the fifth inflammation showed itself in the left testicle. The injection was at once discontinued, and the usual remedies for orchitis were commenced with. On the fourth day, the orchitis having disappeared, the injection gr. j. ad ℥j. was resumed, and in a week the patient was quite well. This patient, whom I have not seen since, informed me that he had suffered from inflammation in the same testicle during a previous attack of gonorrhœa, although he had not then been treated by injection: so that the orchitis cannot be reasonably attributed to the injection.—*Medical Times and Gazette, Oct.* 25, 1873, *p.* 461.

AFFECTIONS OF THE EYE.

97.—ON A NEW PHOTO-PERIMETER.

By C. S. Jeaffreson, Esq., Surgeon to the Eye Infirmary, Newcastle-on-Tyne.

[There is considerable difficulty in eye practice in taking an accurate chart of the field of vision with rapidity. In hospital practice most surgeons continue the old custom of making the patient fix his eye upon their own, and then rapidly moving the fingers in the different parts of the field of vision. This practice is rapid, but must leave a doubtful impression on the mind of the surgeon.]

The instrument I have devised, and which is represented in Fig. 1, enables the operator to map out the field of vision with as great rapidity as, perhaps greater than, it could be done according to the old method described above, and which is in general vogue. With its help, the greatest accuracy can be obtained; and, if necessary, the limits of the field can be immediately transferred to a chart, and kept for future reference. But it not only fulfils the duties of a perimeter in an ordinary sense; it will also test the field of vision for colours, transmitted or spectral; and, by a very simple addition, it can be made to indicate the presence of astigmatism, and show the meridian in which this exists, if present. Some other indications it can fulfil, which I shall speak of by and by, as they will be more readily understood when the instrument has been fully described.

The perimeter is a hollow hemisphere of light metal (zinc or copper), hammered out with great accuracy, its diameter being twenty-four inches. The concavity of the hemisphere is painted white, and divided into degrees of latitude and longitude. The meridians of longitude commence at the pole, and between each is a space equal to 15 deg. The parallels of latitude encircle the pole; the first being at 10 deg. from it, and each successive one placed at an interval of 10 deg. The pole itself is occupied by a circular opening about three-quarters of an inch in diameter, the purpose of which will be shortly explained, as will also some further markings on the surface of the perimeter.

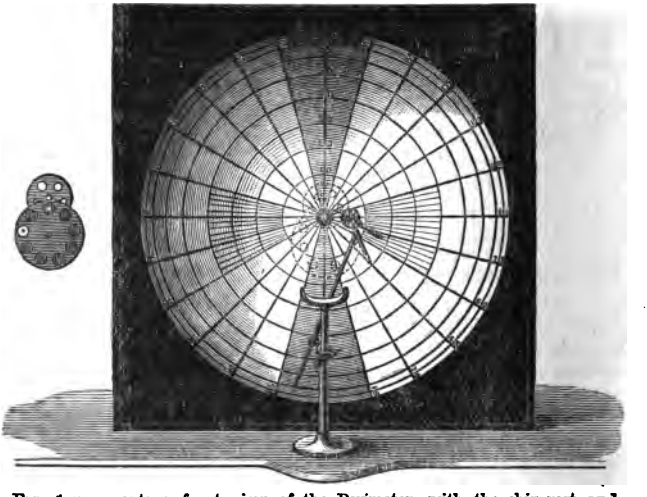


FIG. 1 represents a front view of the Perimeter, with the chin-rest and mirror *in situ*.

FIG. 2 (side figure). Upper part represents the Diaphragm, containing various sized apertures. The lower circle contains the Colour-Discs: these are attached behind the Perimeter, and are drawn in Fig. 1 with dotted lines.

In order to measure the field of vision, it is necessary, whatever the instrument employed, to have a fixation-point, and an object which can be made to travel over the parts of the retina which do not occupy the fixation-point. In the instrument which I am describing, these two indications are fulfilled in the following manner: A gas lamp upon a movable upright stand is so adjusted behind the perimeter, that the rays from it pass through the aperture which occupies the pole of the hemisphere in a diverging beam. These rays fall upon the mirror, which is (as seen in Fig. 3) attached by a rod to the chin-rest; and this

mirror, being ground to a focus of a little less than twelve inches, will form a small and bright dispersion-circle upon the surface of the perimeter. To enable this disc of light to be thrown in any direction required, the mirror is attached to its connecting rod by a ball-and-socket joint. To use the perimeter for testing the field of vision, we proceed as follows. The chin-rest is placed opposite the concavity of the hemisphere; its height is so adjusted that, when the patient's chin is placed upon it, the eye to be examined is opposite the aperture at the

FIG. 3 shews the patient in position for taking the field. The position of the mirror is not quite exact: it should be nearer and more to the side of the eye.

polar extremity of the hemisphere. (Fig. 3). The reflecting mirror is now adjusted and made to occupy a position slightly below and to the outside of the eye to be examined. The other eye is covered by a shade. By a little management, the beam of light, which passes through the aperture, in the perimeter, is made to fall, not directly upon the eye which it would dazzle, but upon the mirror which reflects it, as before said, in the form of a bright dispersion-circle, upon the perimeter. The light, not falling directly upon the eye, renders the fixation-point less dazzling, and more easy to look at for some length of time.

When a patient is placed in the position above indicated, his eye being directed upon the central aperture, an image of the perimeter is projected upon his retina; and it is, so to speak, artificially mapped out into exactly the same divisions as are present on the instrument—the central aperture lying exactly at the yellow spot, the peripheral parts of the instrument occupying the peripheral parts of the retina. The concavity of the retina becomes, therefore, an exact counterpart (of course inverted) of the concavity of the instrument. Now, it is evident that, if a disc of light be made to travel over the surface of the perimeter, this will also travel over the surface of the retina (in an opposite direction to that in which it is passing over the instrument); and that, if it should come to a portion of the retina whose sensibility is lost or impaired, or to the natural limit of the sensitive retina, we shall know by the degrees of latitude and longitude the exact bearings and position of the spot whose sensitiveness is impaired, and where the natural limit of the sensitive retina terminates. Let us illustrate this. A patient is placed in position, with his right eye opposite the perimeter, fixed upon the central aperture spot; we wish to examine the spot or optic nerve entrance in this eye; we know that its situation anatomically is slightly to the inner (nasal) side of the yellow spot; we therefore gradually move the travelling disc outwards; and, when it arrives at a position about 15 deg. to the outer side of the central aperture, it disappears.

If we wish to ascertain the limits of the field of vision, we cause the disc to travel along the various meridians of longitude, marking the exact position in which it ceases to be visible. As in many cases it is necessary to measure the exact size and limits of the optic nerve entrance, the surface of the perimeter, in the region which this portion of the fundus occupies, has been carefully divided into spaces of 3 deg. each.

In order to enter the cases examined, charts of the perimeter can be obtained, drawn up on a scale of half or a quarter the real size. Attached to the back of the perimeter, will be found two sets of diaphragms (Fig. 2). One, the inner, contains apertures of various sizes; and the other, the outer, contains discs of variously coloured glass; also a disc which has a fine slit and revolves upon its own axis, for testing for astigmatism.

The use of these parts will now be explained. Of course, in many cases, where the sensibility of the retina is much impaired, a small aperture and a small disc of light will not be available for measuring the field; hence the large apertures. The smaller, especially the smallest, is for taking exact measure of the blind spot, the area of which can more easily be measured by a fine point of brilliant light. The intensity of

the light may be varied by regulating the amount of gas burnt.

To test the field of vision for colour, we proceed in exactly the same way as under ordinary circumstances; the glass discs of the colour required being interposed between the flame and the aperture in the perimeter.

The processes above described only enable us to test what is termed the quantitative field of vision, or that portion in which a person can distinguish roughly a spot of light. To measure the quantitative field of vision, a stencil letter is inserted in an inverted position in the aperture of the perimeter. Immediately the mirror forms a bright erect image of it upon the perimeter; and the operator thus ascertains the distance from the fixation-point, in different directions, at which the patient still recognises this letter. This forms a severe but very useful quantitative test, and the shape of the test-object can be varied *ad infinitum*.

I must now show how the instrument is used in the diagnosis of astigmatism. In astigmatic persons, as is generally known, all the rays of light falling upon the eye (owing to some changes, congenital or acquired, in the refracting surfaces) are not brought to a focus on the retina at the same point. Hence if an object made up of radiating lines, like a wheel, be looked at from a distance, all the lines will not be distinctly visible. If the refraction of the eye be normal in one plane, the lines which lie at right angles to that plane will be distinctly visible, whilst those occupying it will be indistinct, or perhaps almost invisible. Now these changes, which are well marked when looking at a drawing such as we have named, become much more obvious when a luminous streak is employed. In the perimeter, a diaphragm with a slit equal in thickness to No. xx. Snellen, is placed in the aperture at the back of the perimeter, and is illuminated with the gas through an intervening portion of ground glass. This slit can be made to revolve upon its own axis, and thus pass through all the planes in which incident rays can fall upon the cornea. If the sight be normal, the slit at twenty feet has a well defined margin in all directions; but if astigmatism be present when the slit is passing through the plane at right angles to which it exists, its contour will become blurred, and assume a shape which will vary according to the degree of the astigmatism. Thus, supposing we produce an artificial degree of myopic astigmatism in the horizontal direction by placing a + 30 cylindrical lens before the eye with its axis occupying the vertical meridian, and we then rotate the slit in front of this eye, we shall observe that when it occupies the horizontal direction its form of a slit will be retained, though it may appear somewhat elongated horizontally, but when it occupies the vertical meridian

it assumes the form of a thickened band. A small point of light may be used, if required, for the same purpose, by bringing the smallest aperture of the diaphragm into the centre of the perimeter. The value of testing the field of vision for colour is greatly enhanced when the pure colours of the spectrum are employed; and I find that, with sunlight, this can very readily be accomplished. In a darkened room, a beam of sunlight is allowed to enter by means of a slit in a shutter; this beam is made to fall upon a prism, and a large spectrum is immediately formed. The perimeter is so adjusted that the band of any one colour in the spectrum shall fall upon the central aperture at the back of the perimeter; the rays of this colour will then pass through and be reflected by the mirror in the same way as usual, forming a disc of the colour which at that point corresponds to the back of the central aperture. A spectrum, when necessary, can be produced by artificial light, and then thrown upon the back of the perimeter; but, although it answers its purpose fairly, it cannot compete with the light of the sun. In the vertical meridian, it will be observed that the perimeter is marked off by fine lines into spaces each of one degree. This is to enable the operator to mark the distance of the double images from each other when diplopia is present. To use the instrument for this purpose, the perimeter is placed upon its side, so that the vertical meridian becomes the horizontal one. By means of a wand, the patient is made to indicate the position of the second image, and the number of degrees which separate it from the central image will indicate the angular displacement.—*Brit. Med. Jour.*, *Dec.* 27, 1873, *p.* 752.

98.—CASES ILLUSTRATING THE TREATMENT OF AMAUROSIS BY HYPODERMIC MEDICATION.

By Dr. C. Bell Taylor, Surgeon to the Nottingham and Midland Eye Infirmary.

Case 1.—Henry H., aged twenty-five, moulder, Rock Valley, Mansfield, was led into my consulting-room by his mother. He appeared quite blind, and could not distinguish large articles of furniture, such as mirrors &c., with which he was in close proximity; he could not guide himself, though he made out with some difficulty the large "A" of Snellen's type, a letter four inches long. On examination I found he was suffering from simple white atrophy of the optic discs in both eyes, and at once injected the twelfth of a grain of nitrate of strychnia under the skin of the brow; this was done night and morning for four days. On the evening of the fourth day he was rather behind time, and apologised by saying that he had been to see

his mother off by the train, as he had no further occasion for her services; in fact, his sight improved so rapidly that he desired to return to work at the end of a week's treatment; I persuaded him, however, to stay a fortnight longer, after which he resumed his occupation. This young man had never learned to read, but he could readily make out letters of "pearl" type and see a considerable distance after a fortnight's treatment. He had been blind for six months, and there was no constitutional cause or cerebral defect to account for his loss of vision.

Case 2.—Thomas A., aged twenty seven, private in the 14th Hussars; discharged from the service on account of imperfect sight two years ago. At the time of his discharge he could just guide himself, but could not see to salute his officers. After his discharge he got gradually worse, until he applied to me on Friday, June 12th, 1873, when I found that he could not distinguish the large "A" in Snellen's type, that he required a guide, and that, in fact, although he had good perception of light, all useful vision was abolished. I at once treated him by the hypodermic injection of strychnine, and he speedily recovered sufficient sight to walk about alone, and was able to earn a little money by touting for a photographic establishment. After five week's treatment he had improved so much that he was able to take a situation as groom with my friend Dr. Forbes, of Eastwood. Here he remained until the meeting of the British Medical Association in London in August last, where he was examined by several gentlemen. In London he joined some of his former associates in the army, did not return to his situation, and I have not been able since to hear any tidings of him. The case was one of simple white atrophy of the optic discs—a diagnosis confirmed by my friends, Mr. Bader, of Guy's Hospital, and Mr. Brudenell Carter, of St. George's, who examined the patient with me.

Case 3.—Chas. E. F., aged thirty-five, from Uttoxeter, captain of a merchant vessel. Sight began to fail in July, two years ago, when at sea between Liverpool and London. On arriving in port he consulted Mr. Walker, of Liverpool; was somewhat improved at first, but was ultimately obliged to give up his profession. When I first saw him he could not see across the road or read anything less than letters a quarter of an inch in length. The lateral field of vision inwards was very limited, and there was partial white atrophy of both discs revealed on ophthalmoscopic examination. He was treated by hypodermic injections of strychnine twice daily for five weeks, after which I taught him how to use the hypodermic syringe himself, and he returned home, able to read Bradshaw or "diamond" type, and with excellent distant vision. This gentleman, who had quite given up his profession, has resumed work and gone to sea again as captain of a vessel.

It will be seen by the above cases that the treatment of amaurosis by hypodermic medication occasionally yields results almost as brilliant as those sometimes obtained by surgical operations. In many, however, apparently exactly similar we are doomed to disappointment, and it is impossible to say beforehand to which category any particular case may belong.—*Lancet, Dec.* 13, 1873, *p.* 838.

99.—TREATMENT OF INFANTILE PURULENT OPHTHALMIA.

By J. W. HULKE, Esq., F.R.S., Ophthalmic Surgeon to the Middlesex Hospital.

[The mothers of infants attacked with this disease have so commonly at the time of their confinement a puriform vaginal discharge that it seems more than probable that the ophthalmia is often due to the direct inoculation of the child's eye with the maternal discharges. For the production of the ophthalmia there need not be gonorrhœa.]

It would seem almost superfluous to tell you that you will not wisely employ alike in every case one inflexible mode of treatment. Ever keeping before you a guiding principle, you must always adapt your measures, whatever these may be, to your patient's circumstances. Thus, topical applications, which can only be safely applied by the skilled hand of a competent medical attendant, are out of place where, as in many country districts and in large poor law and club practices, great distances and large demands on the practitioner's time make it impossible for him to see the patient daily. For this reason we seldom use the pure or diluted solid stick of nitrate of silver—a remedy recommended by foreign surgeons. This, again, is the reason why ice, an auxiliary much in favour with our foreign *confrères*, is so little used by us in these cases, because, unless the ice or ice-water is constantly applied, it promotes a greater reaction —its proper use demands such incessant attention as few patients in the working and in the lower middle classes can bestow or command; besides which, ice is mostly beyond the reach of the poor in towns, and not generally procurable in the country. The treatment which has been followed for more than a quarter of a century with much success at the Royal London Ophthalmic Hospital, the oldest and largest institution in this country specially devoted to eye diseases, consists in the frequent use of alum-water (generally gr. vj. ad ℥j. aq. dist.). First, the eye must be thoroughly cleansed from the discharge; and, as this cannot be done without some pain and struggling, it is well for the safety of the child's eyes, and in order to lessen the chance of the accidental inoculation of the nurse's

eyes, to have the child completely under control. For this, it is neither necessary nor proper to give chloroform. A simple and efficient way is to place the child with its arms straight by its sides upon a shawl or on a long towel, and then swathe it round a few times in this, leaving only its head out. So swathed it cannot move, and one person, unassisted, can do all that is required to the eyes. The eyelids being now gently separated without pressing on the eyeball, the discharge should be wiped away, and the eyelashes cleansed with tepid water and small pieces of rag, which should be immediately burned. Next, if the nurse is intelligent and has not clumsy hands, the conjunctival pouch under the upper and lower eyelid should be carefully syringed out with tepid water; for this a common pewter squirt will do. When the pus is thoroughly removed, some of the alum-water should be dropped into the eye, and diffused beneath the eyelids by moving these lightly over the cornea, or it may be injected under them with the squirt. After this the eyelids are dried, and a little simple ointment is smeared along their edges in order to prevent the eyelashes becoming glued together. If the ointment is softened to the consistence of cream by warming it, it may be very neatly applied with a brush. In an acute case the use of alum-water should be repeated frequently. At first every second hour will not be too often, and then as the ophthalmia yields the intervals may be increased, until, in a couple of days or so, six or four applications in the twenty-four hours will suffice. If the surgeon can see the child daily, and he prefers to use nitrate of silver, the eyelid should be everted and cleansed in the same way, and the exposed conjunctival surfaces should be lightly wiped with the caustic-stick, or (which I think preferable) brushed with a solution of the salt—gr. x. ad ℥j. One such thorough application a day is enough, and in the intervals a little alum-water, pure or mixed with ext. belladonnæ (in the proportion of ℥viij. ad ʒ j.), may be dropped three or four times into the eyes. The more powerful astringents or stimulants are to be used at longer, the milder at shorter, intervals. If you employ alum-water in the way I have described, you will rarely have to regret the loss of an eye. Such an occurrence is quite exceptional, and it seldom happens unless the cornea is already sloughing when the child is first brought to you. Implication of the cornea does not necessitate the discontinuance of the astringent remedy; its use should be persevered in, because in the speedy arrest of the ophthalmia lies the best hope of averting or limiting the destruction. A small hernia of the iris through a limited breach in the cornea may generally be left to itself; it will nearly always shrink as cicatrisation proceeds. Surgical assistance is seldom required

except a considerable part of the cornea has perished, and a large piece of the iris, perhaps distended by the lens, protrudes. Under these circumstances the removal of the lens will hasten cicatrisation, and give a better scar; the eye is greatly damaged or wholly lost as regards sight. For this no special operative skill is required. Chloroform may be given; the iris should be cut with a thin, sharp knife, on which the lens will escape—if not, it may be removed with any small convenient scoop. After this the eyelids should be closed, and a small pad of cotton-wool secured on them with a roller. This is only exceptionally required. Not unfrequently, however, young children from two to four years old will be brought to you who have suffered from infantile purulent ophthalmia, and have the cornea replaced by a bulging opaque white scar, termed an anterior staphyloma. Such a prominent scar may prevent the eyelids closing, it is very liable to become inflamed, and it is very disfiguring. On these grounds the abscission of the front of the eyeball may be required. For this anæsthesia is almost necessary. The eyelids are held apart with a speculum, and the eyeball being fixed with a hooked forceps, a small puncture is made into it with a pointed knife behind the line of the ciliary processes, large enough to admit one blade of a scissors, with which the bulging part is cut off, care being taken that the line of the incision lies just behind the ciliary region. The lens is generally included in the piece cut off, but if not it must next be removed. If towards the horizontal equator of the eyeball the line of the incision is taken a little further backwards in the sclerotic, the lower and upper half-circles will fall together more neatly, and will not form the troublesome angle resulting from a truly circular abscission, which sometimes makes the fitting of an artificial eye difficult, and its use painful. In little children I never sew up the wound, and seldom apply anything but water-dressing.

An occasional and very troublesome sequel of infantile purulent ophthalmia is inflammation of the lachrymal sac. I think it is more frequent where an acute ophthalmia instead of thoroughly ceasing has lapsed into a subacute chronic form. If the sac suppurates the abscess cannot be opened too early, and if after the subsidence of the inflammatory swelling, epiphora and a chronic mucous discharge continue, the lower lachrymal punctum and canaliculus should be slit up into the sac. Through this the contents may be gently pressed out several times daily; and once a day after emptying the sac in this manner a few drops of a solution of sulphate of zinc (gr. ij. ad ℥j.) may be dropped into the corner of the eye. If the nasal duct is strictured you will best adopt an expectant treatment until the child is older.—*Med. Times, Dec.* 6, 1873, *p.* 629.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

100.—ON CHLORAL AS AN ANÆSTHETIC DURING LABOUR.

By Dr. W. S. PLAYFAIR, Professor of Obstetric Medicine to King's College Hospital, and Examiner in Midwifery to the Royal College of Surgeons.

The means at our disposal for lessening the sufferings of our patients during labour must always be a subject of great practical interest to the accoucheur. The administration of chloroform during the second stage has become so established a custom among many that it is perhaps hardly necessary to say much with regard to it. The more experience, however, I have of its use, the less, I feel bound to say, do I like it as an anæsthetic during labour; and this, not because it does too little, but on account of its tendency to do more than we wish. While in certain cases, when given with judgment, only during the pains, and not until these have become strong and forcing, it answers admirably, soothing the patient's suffering without retarding her labour or producing complete anæsthesia; in others, it has an unquestionable tendency to diminish the force and frequency of the uterine contractions. I know not what may have been the experience of others, but my own certainly is that in a large number of cases it has a very marked effect in diminishing the strength of the pains, and thereby very materially lengthening the continuance of the labour. Over and over again, when the administration of chloroform has been commenced, I have observed the character of the pains completely to alter, and again recover their former efficiency as soon as the inhalation was suspended. Besides this, I have no doubt that a very continuous use of chloroform during labour has a marked effect in predisposing to post-partum hemorrhage, inasmuch as the tendency to undue relaxation of the uterine fibres continues for a time after the birth of the child. Although I by no means intend by these remarks to advise you not to use chloroform during labour, I certainly do think that it ought to be given with a greater degree of caution, and perhaps more sparingly, than the recommendations in many of our text-books would lead you to believe to be needful. The susceptibility of

patients to its action seems to vary much, and therefore it is all the more necessary that its effects should be carefully watched in each individual case, and the amount administered regulated accordingly.

While, in my judgment, chloroform is apt to be too freely and incautiously used, the administration of chloral as a means of lessening the pains of labour is, I think, by no means as yet appreciated at its proper value. It has this immense advantage over chloroform, that it does not seem to diminish the strength and intensity of the pains, while it very markedly diminishes their painfulness. It has also another great recommendation, that it is chiefly applicable at a period when we would not think of administering chloroform—towards the termination of the first stage of labour, before the complete dilatation of the os, and when the sharp grinding pains perhaps produce more suffering and are less easily borne than the more forcing pains of a later stage. There is a type of labour very common, especially in women of a highly-developed nervous organisation, such as constitute a large proportion of our patients among the higher classes, in which I have found it to be specially valuable. In these, before the rupture of the membranes and the complete dilatation of the cervix, the pains are very severe, but short and ineffective, chiefly limited to the back, and producing little or no effect in dilating the os. Hours and hours of really intense agony often elapse, until the patient is wearied and exhausted by her fruitless sufferings. In cases such as these, a common and very useful practice has been to administer a considerable opiate, so as to produce some hours of refreshing sleep, after which we expect the labour to recommence with fresh vigour and effect. The disadvantage of this plan, however, is that during the action of the remedy the labour is suspended, and much time is thus lost. If, however, chloral is administered instead of the opiate ordinarily employed, the probabilities are that the same refreshing rest will be obtained without any suspension of the pains or protraction of the labour. The character of the uterine contractions will be observed to alter; they will become steady and useful, but they are not suspended. Another condition frequently associated with the former is rigidity and spasm of the cervix. Very generally in this class of cases the cervix is thin and rigid, with a sharp edge. Soon after the chloral has taken effect the tissues seem to relax, and I have not unfrequently observed a thin os, which had remained unaltered in character for many hours, dilate rapidly under the influence of the remedy, far more so than when chloroform is inhaled for this indication. It is not, however, only in cases of this kind, which may be classed among abnormal labours, that the use of the drug is of value, although it finds perhaps in

them a more special application. It may, I think, be very generally and advantageously exhibited in perfectly natural labour, for the specific purpose of lessening the sufferings of the patient. When judiciously given the patient falls into a drowsy state, not quite asleep, but nearly so. She is roused as a pain begins, but suffers comparatively little; and experienced women, who have the recollection of former labours to guide them, bear strong witness to the immense relief thus obtained. I have given the remedy in this way for the past two years in most cases I have attended, and I have no reason to think that any bad effects have followed its administration. I have very carefully watched the intensity of the contractions, and I have not the least ground for thinking that it has any effect in diminishing either their frequency or their force.

The way I give the drug is as follows:—I order a six-ounce mixture, containing a drachm and a half of the hydrate of chloral. When the pains are becoming severe, and I deem it advisable to employ the anæsthetic, which is generally not until the first stage of labour is approaching completion, I give one-sixth part of the mixture,—*i.e.*, fifteen grains of chloral. This I repeat in about twenty minutes; and usually after the second dose enough has been taken to bring the patient sufficiently under the influence of the remedy. Its farther administration must now be regulated by its effects. If the patient is drowsy and relieved, a third dose need not be given for three-quarters of an hour or an hour; and then half the quantity will probably suffice to keep the patient in a sufficiently somnolent state. It is seldom necessary to give more than a third dose; and I have never given more than a drachm of chloral during the entire labour. In this way, lessening the quantity after the second dose, and increasing the intervals between their administration, a full and sufficient effect can usually be kept up for many hours. I feel certain that any who give this method a fair trial will appreciate its value.

The exhibition of chloral in this way is no novelty. It has, I believe, been recommended more than once in our journals; but, so far as I know, it has never come into anything like general use as an anæsthetic. Bear in mind that it need not at all interfere with the exhibition of chloroform. When the pains get strong and forcing, that may be inhaled just as if chloral had not been given, only a smaller quantity will probably suffice. As our patients suffer less, they are also less urgent in their demands for the commencement of the chloroform inhalation; and thus there will be less likelihood of those evils I have mentioned to you being produced.—*Lancet, Feb.* 21, 1874, *p.* 263.

101.—THE ADMINISTRATION OF CHLORAL IN THE FIRST STAGE OF LABOUR,

By William Berry, Esq., Manchester.

On August 31st, 1870, I was summoned to attend Mrs. A., aged thirty-five years (primipara). I saw her about ten p.m., and finding she was suffering from the sharp, short, and grinding pains peculiar to the commencement of labour, the os not admitting more than the tip of my index-finger, I gave her forty minims of tincture of opium, and left her. I was sent for again an hour afterwards, but found no improvement as far as regarded the progress of the case. I ordered thirty grains of chloral every half hour until relieved. Three doses had been given when I was sent for again (this would be about one a.m., and ninety grains of chloral had been taken). I found the os was dilating, the head could be felt presenting in the first cranial position, the membranes were entire, and the pains were regular and pretty strong. The pains continued, and got stronger as labour advanced; the os was fully dilated, and membranes ruptured about 3.30 a.m. The pains gradually grew stronger, and the child was born about five a.m. Placenta came away easily, and both mother and child did well. During the whole of the time I was with her she slept soundly between each pain. The accession of a pain was marked by being aroused, and the efforts made at expulsion with a slight groan. When the pain had passed off she again relapsed into a slumber, which continued during the interval.

During the whole course of the labour there was nothing to lead one to suppose that the chloral had retarded the progress of the case. On the contrary, the dilatation of the os, the descent of the head, the character and frequency of the pains, were all that could be desired, and there appeared to be great relief of suffering.—*Lancet, April* 11, 1874, *p.* 532.

102.—A FEW REMARKS ON THE HYPODERMIC EXHIBITION OF ERGOTIN.

By Dr. Francis E. Clarke, M.A., Drogheda.

I have lately, in two cases of severe hemorrhage occurring with abortion in the third month of pregnancy, been led to try the effects of ergotin injected subcutaneously, and being very favourably impressed with those produced, though in so very limited an experience, I think, perhaps, it might possibly be interesting to the readers of the Medical Press and Circular, briefly to bring the topic under their consideration. In each case that I adopted this procedure, hemorrhage had occurred of such magnitude as to completely prostrate the patient and imminently

imperil life. Two injections in both instances were employed. In the first case, a grain of Bonjeau's ergotin in ten minims of distilled water twice, at an interval of seven hours prior to the expulsion of the ovum, each exhibition appearing completely to control the hemorrhage; and during the intermediary period there was no hemorrhage whatever. In the second, almost total collapse had accrued; hemorrhage had existed eighteen hours; the vagina had been carefully and most completely plugged; one ovum had come away and yet the hemorrhage continued. The pulse was imperceptible in both wrists; the stomach failed to retain even iced-woter. Everything imaginable had been tried, when I injected subcutaneously, a grain and a half of ergotin in eleven minims of distilled water. Not fifteen minutes had elapsed before plug and a second ovum were forcibly extruded and hemorrhage immediately subsided. About eight hours subsequently flooding again commenced, owing, most likely, to relaxation of the uterus, and perhaps retention of some portions of membrane. Again I injected the ergotin, this time two grains with equal success, toward effecting control of the hemorrhage. In case A, the skin about the seat of puncture remained inflamed and tender for a day or two. In case B, no discomfort whatever ensued. This seems to have been a source of inconvenience experienced by many of those who have hitherto resorted to the hypodermic method of administering ergot. Indeed, were it constant, it would be a serious objection to the practice in any but cases of the most exquisite emergency. The absolute purity of the drug employed and its solvent, the scrupulous cleanliness of the needle and its penetration sufficiently far enough to have the aperture freely under the skin in the subcutaneous cellular tissue, all seem to me to warrant the most scrutinising attention on the part of the operator. Hildebrand, of Königsberg, when he first suggested the subcutaneous employment of ergotin for the removal of fibrous uterine tumours, added glycerine to the formula. The liquid extract of ergot with glycerine has also been used. I cannot help thinking, however, that the simpler in form all fluids be prepared for hypodermic use, the better, and surely the less irritating; though doubtless, cherry-laurel water has lately been suggested as a vehicle on several grounds, amongst which are its non-irritating properties when in contact with the cutaneous structures. Langenbeck and Eulenberg first called attention to the subject, and I think it was ergotin they employed. It is a matter of the utmost importance to determine the most efficacious and certain form to administer the active principles of ergot hypodermically, if a more prolonged experience and its favourable reception by observers of competency should lead us to adopt this method of administra-

tion more extensively. Mr. Long, in the interesting paper published in this journal in 1870, at the time when his then new and admirable "liquor ergotæ" was being introduced, tells us how widely different are the samples of ergotin to be found in commerce. He states that Bonjeau's is an aqueous extract, excluding the poisonous oil previously thought by Dr. Wright to be the active principle, but really the agent which produces all the dangerous symptoms of that state commonly known as ergotism. If such be the case, one would decidedly imagine it to be superior to anything else of the kind. Mr Long's liquor ergotæ, I opine, is admitted now to be the very best form for the internal administration of ergot. For my part, I don't see any other equal to it on several grounds, but it is decidedly inapplicable for subcutaneus exhibition. That the alkaloids, morphine, atropine, strychnine, are more active with respect to their physiological characters when exhibited hypodermically, there can be no question; and if ergot, likewise exercising specific power over the excito-motor functions of the spinal cord, and hence capable of modifying the contractility of the arteries generally, is similar, we have undoubtedly another great therapeutic addition to our assistance. In addition to the superiority of its action when so administered, if such superiority exist, we meet with many cases where it is impossible to administer it by the mouth; and secondly, the nausea and vomiting so frequently thus produced, might not be found to follow it subcutaneously. Its action also seemed to me to be quicker, to follow more promptly, and hence, better suited it to cases of emergency, such as those severe uterine, gastric, pulmonary, or nasal hemorrhages that are occasionally to be met. Regarding gastric irritability, it appears rather to subside than otherwise after using the ergotin. Of course, I cannot say that this was consequent thereon, but it strikes me very forcibly that it tended to supplement the action of the spinal ice-bags I had applied, with the view of modifying the reflex excitation of the spinal cord as suggested by Dr. Chapman, ergot being generally admitted to contract its vessels and diminish the afflux of blood. There is no more effectual treatment for obstinate vomiting, hiccough, or such like alarming phenomena than the hypodermic administration of morphia over the epigastrium—frequently succeeding when everything else fails; yet this seems paradoxical, when we remember how opium congests the cord, and hence might be expected rather to aggravate the visceral irritation than otherwise, by increasing its polarity and the consequent reflex excitability. This difficulty may be overcome either by showing that the physiological powers of opium over the spinal cord are not possessed by morphia or its salts, or else by adopting the

hypothesis that when used in such cases it produces hypodermically only a localised action, causing relaxation of the abdominal muscles and a consequent quiescence of the internal viscera in proximity; but surely, this must be through the medium of spinal influence. The mechanical effects resulting from the morphia so administered predominate over the physiological. The removal of spastic pressure by the relaxation of the muscles, has greater power in suppressing vomiting, than the physiological effects over the cord have to the contrary; but in the case of ergotin, even if the diminished spinal vascularity is not sufficient to counteract peripheral nervous reflex excitability, it is possible that it may be regarded supplementary to the ice-bag, if used with that intention, and in any case capable of administration where its gastric retention is out of the question, as well as in the treatment of any great hemorrhages, such as uterine, pulmonary, &c. In subcutaneous application of ergot, if it proves on further trial than I think has as yet been accorded it to be worthy of reliance, we would have another means at command for "the anticipation of post-partum hemorrhage," lately so ably discussed by Dr. Lombe Atthill in the British Medical Journal, and in cases of such, owing to exhaustion of the nervous system of the mother, as laid down by that author, to be more difficult to anticipate, hence, to treat, than simple exhaustion of the uterus, immediate action of ergot (if so obtainable) would be highly important, and small quantities of strychnine, suggested by Dr. Atthill, could with equal facility be added to the hypodermic solution as to one for internal administration, and with a quicker and, most probably, a more powerful result. The hypodermic syringe is comparatively a recent introduction—the suggestion of ergotin so applied still more so; we have, therefore, to guard against being led away by novelty. Our experiences, however, so far justify continued trial, in which neither prejudice on the one hand, nor enthusiasm on the other, should be allowed to mislead the calm and reflective judgment of a thoughtful and unbiassed investigation.—*Medical Press and Circular, Jan.* 28, 1874, *p.* 65.

103.—ON A NEW OBSTETRIC FORCEPS.

By Dr. H. LOWENTHAL, New York.

Some of our most distinguished writers and teachers on obstetrical subjects have recommended that the study of the forceps be more cultivated, because they are the means of saving in extraordinary cases one or two human lives. The consequence of these recommendations has been the invention of a host of

new forceps, all for the purpose of facilitating the application and of increasing the effect of this valuable instrument.

Some made a new lock, others changed the length of the blades, the size of the shafts, the move of the handles; others, again, thought small fenestræ in the blades would be more efficient, or broad blades would hold the fœtal head more securely than narrow ones; others finally changed the curve of the blades, made two curves, or one curve, or none at all, and so on. But to show to the profession that those hints were not lost on me, I tried to find out how it were possible to apply a forceps without subjecting the mother (and often the operator) to the difficulty of applying the second or right blade and locking the forceps; in other words, I tried to find a forceps whose blades could be introduced at once, and by which the head of the child could be caught by rotating one blade around and away from the other; and I give you in the accompanying cut an idea of the instrument I had made. I have tried it on the phantom, and finding it perfectly successful, I offer it to the profession, knowing full well that there is a great deal of room for improvement, as regards the length and width of the blades, and the curves of the same.

I shall now endeavour to show in what way my instrument is superior to all the others.

I do not think that there is any necessity for speaking of the effect of the obstetrical forceps in general, since almost every medical journal of the last year or two contains essays on the same subject, written by more able and more experienced authors, but I shall only mention that just these essays brought me to the idea of having my forceps constructed.

The forceps should act as tractor, as compressor, and as lever, and I think the arrangement of the lock of my forceps meets these claims best of all.

The lock, being solid and stationary, is more apt than any other to procure for the operator a steady traction; the blades, working around a joint, will give a higher degree of compression than any other (without being too much in that direction, because if the head were too large, the rotation of the blades could not be accomplished), and the whole instrument being straight, and the handles, working as it were like one piece, will give a greater amount of leverage than any instrument the handles of which have to be compressed by the hands of the operator in order to steady the instrument.

So much in regard to the general application of the forceps; but there are a few new points to be remarked in my instrument, which are well worth being mentioned. First of all, it does away with the introduction of the second blade. Every medical man who has been called upon to apply the forceps will agree

with me that the greater difficulty is always in the application of the second blade and the locking of the instrument. After you get the first blade in position, you will have to intrust its handle to some assistant (usually an inexperienced woman) to hold and steady it, and at the same time keep it out of your reach, not to hinder you in applying the other blade. While you are busy in doing so, this assistant will move or let go the handle, and thereby necessitate adjustment of it again, before you can proceed with your work. Finally you have both blades in position, and try to lock them, but you find some difficulty in doing so; you will have to move either the first or the second blade, and sometimes not with inconsiderable force, thereby risking the making of abrasions on some part of the child's head. All these difficulties are done away with: you introduce your instrument as you do the first blade of any of the other instruments. There is usually very little resistance; then seize hold of both handles with one hand, and rotate the instrument. either to the right or to the left, either around the anterior or the posterior half of the pelvis, and as soon as the two blades come opposite to each other, with a click the lock will close, and you can commence traction, or use leverage, at your discretion, as the case might be, without fear lest the instrument might open, or the blades might move, and without using any force to compress the handles, as with other instruments. This instrument, as I present it now, could be made of any length, but I intend to use it as a straight forceps, and shall always follow the old rule of putting the mother across the bed on her back, bringing the nates close to the edge, thereby giving the most space to the operator.

In closing this explanation I do not need to give any further description of the forceps, as the construction of the blades, or handles, or shafts, has nothing new, nor is it necessary to give further directions for the use of the instrument, since it is so simple that every practitioner who has ever handled a pair of forceps before, will at once understand the workings of this new one: the only point to be noticed, if you want to rotate the outer blade around the posterior half of the pelvis, is to steady the instrument on both sides at the horns of the handles with one hand, and rotate the instrument by grasping the lower end of the handles with the other; on the other hand if you want to rotate the inner blade around the anterior half of the pelvis (behind the os pubis), you steady only the one handle and rotate with the lower end of the other.

[This forceps resembles in its principal features one described by ourselves at the meeting of the British Medical Association in Leeds in 1869. (See *Retrospect* vol. lxi, p. 273). We consider that the plan of fixing the handles together so that they

cannot be separated after introduction is disadvantageous, as it considerably lessens the ease with which the blades are glided round the head.—EDS. *Retrospect.*]—*New York Medical Record, Feb.* 15, 1873, *p.* 94.

104.—THE DELIVERY OF THE PLACENTA BY SUPRAPUBIC PRESSURE.

Dr. GOODELL, in the Transactions of the Medical Society of Pennsylvania, writes that, judging from his own experience and from the number of laudatory papers on this subject, Credé's method of delivering the placenta, or some slight modification of it, bids fair to take the place of every other. The plan which he adopts is as follows. At the maximum of the first uterine contraction after birth of the child, the fundus of the womb is grasped through the abdominal wall, between the thumb in front and the fingers behind. It is then both forcibly squeezed, and at the same time pressed downward and backward. By means of this uterine expression, the placenta and membranes are usually at once detached and extruded; sometimes suddenly, just as the stone escapes when a cherry is compressed between the finger and thumb; occasionally it will require two or more pains to effect this. The sooner this plan is resorted to after the birth of the child, the more easy in execution will it be. Those who practice this method contend that it offers many advantages over any other. The risk of communicating any puerperal disease is lessened. The expulsion of the placenta and membranes by a *vis a tergo* is more likely to be complete than by traction on the cord, which cannot be broken, as no traction is made on it. Adherent placenta is less frequently met with. The introduction of the hand into the womb is avoided, and so also, as a consequence, is the ingress of air. Finally, the tonic and energetic contraction of the womb, following this manœuvre, prevents the occurrence of hemorrhage or of unruly after-pains.—*British Medical Journal, Feb.* 7, 1874, *p.* 175.

105.—CASE OF CEPHALOTRIPSY AFTER TURNING.

By PETER BROTHERSTON, Esq., Alloa, Scotland.

In the following very short notes of a recent case of crushing and extraction by the cephalotribe of the head of a child left in the uterus, I have nothing to state in the way of experience which has not already fallen under observation. From this case, however, those engaged in midwifery practice, more especially in the country, may find their confidence strengthened in

an instrument which has afforded a safe and easy assistance in cases of fœtal decapitation, or any case of difficult delivery requiring the head to be crushed, when other means of remedy might involve a much more tedious and dangerous operation.

Dr. Keiller has related, in the Transactions of the Edinburgh Obstetrical Society, Session 1868-69, a similar case to that of my own, which saves me recapitulating what he has written.

On the 16th of June last, I was called suddenly to assist a friend with a case in which the child's head had become detached from the body during efforts at extraction, and had been left in the uterus. With such instruments as were in use previous to the cephalotribe, the extraction of the head would have involved a tedious process, aggravating the mother's sufferings, running a risk of wounding delicate parts, and incurring delay, which is always more or less dangerous in such cases.

I fortunately had previously seen the cephalotribe in the late Sir James Simpson's hands, and was sufficiently struck with the useful appearance of the instrument, to purchase one at Mr. Gardner's, South Bridge, Edinburgh.

The patient I found rather depressed and anxious, and in no condition for undergoing a lengthened and difficult operation. The contraction of the uterus had ceased, and the uterus, with the enclosed head, had receded into the cavity of the abdomen.

Before proceeding, it was thought expedient to administer chloroform. Carefully inserting one blade of the instrument into the uterus, and then the other, I locked the blades, and proceeded to tighten the compressing screw. On withdrawing the instrument it enclosed the placenta, having slipped from its hold of the head. My friend then placed his hand on the uterus, and exerted sufficient pressure to throw the head more into the axis of the pelvis, and at the same time seized hold of the lower jaw to steady the head. Once more the blades of the instrument were inserted, locked, and the screw tightened. On this trial the head was firmly caught over the ears, and without any difficulty crushed and withdrawn from the uterus.

The antero-posterior diameter of the pelvis was much contracted, scarcely admitting four fingers. I learned that the woman had previously been delivered of a small premature dead child. She afterwards made an excellent recovery, and was moving about in her usual health in ten days.

Having obtained a photograph of the head in the grasp of the cephalotribe, the manner of the action of the instrument is discernible enough. The pressure exerted is sufficient to crush the cranium and face into a frightfully distorted form; but that is of little consequence when so successful an operation can be so easily achieved.—*Edin. Med. Jour.*, *Feb.* 1874, *p.* 715.

106.—THE ANTICIPATION AND TREATMENT OF POST-PARTUM HEMORRHAGE.

Abstracts of Papers by Mr. QUIRKE, Mr. TRESTRAIL, Mr. J. THOMPSON, Mr. TURNER, Mr. HOUGHTON, Mr. GURNEY, Mr. KNIGHT, Dr. BOULTON, Dr. EDIS, Mr. PARSONS, Dr. HADDEN, Dr. DONALDSON, Mr. WOODMAN, Mr. DATE.

Mr. JOSEPH QUIRKE of Birmingham states that he has employed the intrauterine injection of iron as a *dernier ressort* in three cases of *post partum* hemorrhage, with the effect of immediately arresting it in each case, the patients recovering without bad symptoms. In spite of firm and continuous pressure and manipulation of the uterus, the employment of cold, the application of the child to the breasts, the administration of brandy, the introduction of the hand, and the injection of cold water into the uterus, the formidable loss continued, and death was imminent. The injection of a solution of iron (1 to 20) caused the uterus to contract with great energy, expelling the hand that had been passed in, together with a number of clots, not another drop of blood being lost. He considers that the risk of embolism is obviated by the firm contraction of the uterus and the consequent closure of its vessels; and that metritis and peritonitis are not likely to occur, if too strong a solution be not employed. He thinks its employment should be restricted to those cases where all ordinary remedies fail.

Mr. H. ERNEST TRESTRAIL of Harston endorses the views of those who employ ergot as soon as the head is born, and carefully follow down the uterus with the hand as the child is expelled, continuing to support it until the application of the bandage after the placenta is removed, the binder having been placed in position before delivery. Where these means fail, and cold injection is not efficacious, in place of relying on pressure on the aorta or on galvanism, he advocates the injection of a solution of iron or some other powerful styptic, the danger being in using too strong a solution. He prefers chloralum, being less likely injuriously to affect the tissues with which it comes into contact, and being at the same time a more powerful antiseptic. He thinks that ergot is given in too large doses generally, having seen violent shaking, a sense of sinking, sickness, &c., and even symptoms of collapse and thrombosis, as results. Again, one of the most distressing results is powerful contraction of the uterus, producing "continuous pains," and threatening injury to the womb. He has also seen it produce irregular contraction, the cervix acting so forcibly as to interfere with the extraction of the placenta. He considers five to ten minims of the liquid extract of ergot sufficient in all ordinary cases as a safeguard against *post partum* hemorrhage.

Dr. J. Ashburton Thompson thinks that sufficient stress has hardly been laid, if it have not indeed been entirely overlooked, upon the administration of a full dose of ergot at such a stage of labour as, it may be calculated, will allow the drug to take effect just before the final act of expulsion. It should be given twenty or thirty minutes before delivery is expected. He has been in the habit of giving it invariably to every case in which there is no obvious mechanical contra-indication. No harm accrues to the fœtus; and the ergot has on many occasions obviated a flooding which there was every reason to anticipate, and also diminished the severity of the after-pains. If there be any septicæmic element in the case, its influence must probably be diminished by the more efficient and lasting contraction of the uterus. Special precaution should be taken to prevent any sudden evacuation of the uterus after the administration of ergot; otherwise the placenta may be with difficulty extracted. Grasping the uterus by the hand, after immersion in cold water, will both expedite the expulsion of the placenta and also lessen the tendency to hemorrhage. He then gives the outlines of eight cases occurring in his own and Dr. Brunton's practice, in which flooding had taken place in previous labours, but was obviated by the administration of a full dose of ergot in the present deliveries. In one case, the intrauterine injection of a solution of iron was resorted to without permanent effect, continuous pressure ultimately succeeding. Ergot prevented flooding on the next occasion.

Mr. John Sidney Turner of Norwood thinks *post partum* hemorrhage occurs more particularly in women of relaxed habit, where labour is long protracted, and there have been long intervals between the pains in the last stage. He inculcates avoiding haste in delivery, and waiting patiently for the extraction of the placenta, giving ergot prior to the birth of the child. Compression of the aorta should be resorted to where practicable. Galvanism with hand-kneading is the special means which he has found of very decided advantage; one hand being placed in the uterus, and the galvanism being applied externally. He considers this far more philosophical treatment than that by injection, intensifying the physiological mode by which uterine hemorrhage is arrested, and imitating Nature's process in the closest degree.

Mr. J. Hyde Houghton of Dudley, a pupil of the late Dr. Rigby, writes in praise of the binder applied before the birth of the child, and tightened as soon as this takes place. He says that, out of a very extensive midwifery practice, he has had only one fatal case during thirty-three years. During the last eight or nine years, he has carried about a binder with buckles, and has not had a single case of hemorrhage that has given him the least anxiety.

Mr. THOMAS GURNEY of Stoke Newington advocates ergot and pressure by the hand externally on the uterus, together with the application of cold plates dipped in water, or a sponge saturated with cold water introduced into the uterus; failing this, the introduction of a Barnes's bag and its distension with cold water.

Mr. H. J. KNIGHT of Rotherham endorses Dr. Mushet's views on the value of cold-water uterine injection. He has employed it in several cases, and has never seen any ill effects follow this simple and effective mode of treatment.

Dr. PERCY BOULTON replies to Dr. A. B. Steele's strictures on his previous communication respecting the abdominal binder, and states that it is not the *bandage* for which he claims any special advantage, but *perfect rest* after delivery. He expresses also his appreciation of the value of the injection of iron-solution when hemorrhage with inertia of the uterus occurs.

Dr. EDIS thinks that, in the multiplicity of methods of *treatment*, we are apt to lose sight of the importance of *anticipating post partum* hemorrhage, to which Dr. Whittle's original paper and Dr. Lombe Atthill's remarks were almost exclusively confined. Attention to the condition of the health of the patient preceding parturition is as important as the training of the athlete, and yet how seldom is it thought of! Iron and strychnia are of great service in improving the general tone of the general system. "Where we have any reason to expect hemorrhage, every precaution should be taken to obviate this, by refraining from the administration of chloroform except just at the completion of the second stage; by applying the forceps, if there be any unusual delay from inefficient uterine action—not waiting for the natural powers to exhaust themselves; by assisting the expulsive efforts by means of the abdominal bandage; by giving ergot just before the final throes; and by not hurrying the extraction of the child or of the placenta, but following the uterus down with the hand externally, allowing an interval to elapse between the birth of the child and the expulsion of the placenta. Then, again, anticipate your wants; have a second skilled assistant, to attend to the child, and so leave you free to devote your attention entirely to the mother; have plenty of ice as well as ice-water at hand, with a proper syringe and tube, and your iron-solution ready mixed. Have some ergot poured out, and brandy within reach. See that the arrangement of the bed is such as to leave you room to move. Have a hot water bottle, ready filled, to place under the lower back, thus increasing the determination of blood to the spinal cord, and so intensifying the nervous supply to the uterus. Forget not your transfusion apparatus, including a subject able and willing to supply the life-giving stream if requisite; com-

pression of the aorta; ligatures to the extremities; hips raised above the level of the pelvis; free ventilation; and exclusion of all unnecessary attendants. Above all, a quiet and confident manner on the part of the attendant will inspire hope and ensure success; and, if every one who practises obstetrics will *think* beforehand and *act* when called upon in such emergencies, we shall less frequently have to deplore the sad issue of these anxious cases."

Mr. FRANCIS J. C. PARSONS of Bridgwater states that his experience is diametrically opposed to that of Dr. Lombe Atthill and Dr. Steele. He thinks that, the placenta acting as a foreign body, its speedy removal is of vital importance; and the rule he invariably adopts is to remove the placenta as soon as the child is born and the umbilical cord secured. "Placing the left hand on the fundus uteri, and gently moving my fingers backwards and forwards, accompanied by slight pressure, I feel the uterus, thus excited, to contract, and almost immediately the placenta is expelled. I continue my command of the uterus for ten or more minutes, and then substitute a pad in place of my hand beneath the binder, before applied, during the second stage. By these means *post partum* hemorrhage is with me of most rare occurrence." He agrees with Dr. Barnes that "the removal of the placenta is the first great end to be attained as a security against hemorrhage," and refers to the practice of Hardy and McClintock as corroborating this view.

Dr. H. R. HADDEN of Rathmines, Dublin, narrates the particulars of a case where Cæsarean section was performed. "On the removal of the child and placenta, severe *post partum* hemorrhage set in. Here there was no difficulty in the fullest application of the hand to the interior of the womb, nor even to grasping the organ in both hands, and firmly compressing it; but neither this, the application of cold, nor any other means, was of the smallest avail in controlling the hemorrhage or stimulating the organ to contract and close the gaping orifice which my incision had made, till I resorted to the diluted solution of perchloride of iron, when immediately the exact appearances described by Dr. Barnes were seen by the eye; the uterus contracting; the styptic serving to seal up the mouths of the vessels and corrugate the inner surface of the uterus."

Dr. JAMES DONALDSON of Glasgow writes to record his experience of these cases when senior civil medical officer on the Neilgherry Hills, 7,500 feet above the sea-level, where local hemorrhages were common. Epistaxis was frequent, and at times troublesome, even serious and copious oozing ensuing after extraction of teeth; and the ordinary monthly flow in

women, often excessive, became at times dangerously profuse. During nearly four years' tenure of office, *post partem* hemorrhage was of constant occurrence; but he did not lose a single case. Thoroughly alive to the great danger and responsibility, he always went prepared. In one case of rapid labour, "everything went on well, the placenta being expelled, and the uterus contracting. In less than an hour, the patient became suddenly restless and began to shiver. On removing the binder, the uterus was found to be relaxed, and internal bleeding going on. All ordinary means failing, the hand was inserted, and the clots extracted. This had to be repeated thrice, the hand being kept *in utero* for nearly an hour. The patient recovered. In a second case of natural labour, a portion of retained membrane was found to be keeping up hemorrhage, and was removed. In another case, a full dose of ipecacuanha, bringing on rapid emesis, caused strong contraction after inertia, where introducing the hand was not thought desirable. In another case, where the uterus was well contracted, plugging the vagina with sponge steeped in perchloride of iron lotion arrested hemorrhage from the cervix. Where it is necessary to introduce this lotion into the uterus, it is much more effectually and safely done in most cases by the hand guiding a sponge and applying it as required. Many cases are lost where the hemorrhage occurs from the lower part of the uterus and upper part of the cervix, and is unsuspected. The plug with iron lotion is the best and surest remedy for this."

Mr. JOHN WOODMAN of Exeter writes to corroborate the advantages and success of the treatment by free injection of cold water, as advocated by Dr. Mushet. He has tried it in several cases, and always with success, and has found it to produce contraction of the uterus when galvanism and all other means had failed. It is essential that the water be quite cold. Occasionally he has used a little vinegar mixed with the water, but he believes that the good effects are entirely due to the *direct* application of cold to the uterus. He has never had any bad results from this treatment.

Mr. WILLIAM DATE of Crewkerne, Somerset, states that he has attended over two thousand cases of labour, and has never lost a patient from *post partum* hemorrhage. He advocates grasping the uterus and keeping up pressure—kneading it if necessary—until firm contraction ensues; pressure being kept up for an hour, if requisite. A firm broad binder is then applied. He refers to the fact of ergot not being under all conditions absorbed by the stomach, and thus failing to exert its well known action.—*Brit. Med. Journal, Dec.* 27, 1873, *p.* 753.

107.—ANTICIPATION AND TREATMENT OF POST PARTUM HEMORRHAGE.

By Dr. W. Boyd Mushet.

I have watched with interest the remarks and opinions of leading men as to the use and efficacy of perchloride of iron and other means in the control of uterine hemorrhage. I am induced to describe the remedy adopted by myself, as I have not seen it definitely mentioned. I therefore deem it novel. I employ an ordinary stomach-pump or enema apparatus, and *inject cold water freely into the uterus*, passing the long tube well up for the purpose. I throw in the water again and again, having previously introduced my hand to ascertain that the uterus is free from retained fragments of placenta. The fundus should, of course, be grasped externally as firmly as possible.

Where there is a suspicion of the occurrence of hemorrhage, I have the apparatus prepared, water ready in a basin, and a dose of ergot on the mantel-piece; the last given as the head passes the perinæum. In one patient, on two occasions, where the flow, without hyperbole, was appallingly like a cataract, I succeeded in arresting the discharge instantly. In this case, against my wish, chloroform was each time inhaled for some hours, which, I believe, increases the proclivity to hemorrhage.

The virtues of the perchloride of iron are mainly, I think, to be ascribed to its vehicle, cold water, when used with the latter. I have no dread of cold water injections; no compunctious fears of pyæmia or metritis from caustic or irritant influence on the uterine tissues or sinuses, as I have never witnessed any injurious effect from their employment, which cannot be affirmed of the perchloride, if we trust the evidence adduced at a former meeting of the Obstetrical Society.

Whilst on this subject, I would add that, in a protracted case of scirrhus of the uterus, in which periodical attacks of severe bleeding occurred, attended with great anxiety on the part of the patient, I always found injection of cold water more promptly valuable than ergot and less distressing than the plug—so markedly, that the poor emaciated sufferer, to whom an euthanasic dose would have been an act of grace, invariably begged for the injection, in which, after experience of the several remedies, she reposed utter confidence, though it produced very severe temporary shock on her attenuated system.—*British Medical Journal, Nov.* 22, 1873, *p.* 601.

108.—THE TREATMENT OF POST PARTUM HEMORRHAGE.

By Dr. ROBERT BARNES, Obstetric Physician to St. Thomas's Hospital.

The choice between cold and styptics depends upon our appreciation of two entirely different physiological conditions. Styptics will act when cold will not. When the diastaltic system is dead to irritation, when life is almost ebbing away, when no known power can compel the uterine muscle to contract and thus to close its bleeding vessels, styptics, which act chemically, will still seal up the mouths of those vessels and corrugate the inner surface of the uterus.

For want of experience, perhaps, of the more desperate forms of hemorrhage, this fundamental distinction has not made sufficient impression upon some men. I frankly accept their assurance that they have never failed to control hemorrhage by compression of the uterus or aorta, by ergot, or by cold. I hope they will equally accept my assurance that I have seen many cases where men of the highest ability and experience had failed to subdue hemorrhage by these means, and where, at this juncture, the perchloride of iron has instantly rescued the patient from impending danger.

In my *Obstetric Operations*, I have insisted as forcibly as I could upon this classification of cases, because I think that upon it rests the justification for urging a new plan of treatment. So long as cold acts, use it; but, when cold fails, use iron, which will not fail. Each has its use; each must have its turn. In my book, at p. 461, 2nd ed., is the following passage. "Cold is more effectual if applied internally. Levret was, I believe, the first who used ice in this way. Perfect, says Levret, 'hit upon a very odd and ingenious expedient; he introduced a piece of ice into the uterus, which, being struck with a sudden chill, immediately contracted and put a stop to the hemorrhage.'" A graphic clinical description of reflex action before the theory was known! Tyler Smith, to whom the theory and practice were alike known twenty-five years ago, advocated the injection of iced water; and, notwithstanding his early teaching and long experience, this keen observer and sagacious practitioner in the latter years of his life resorted to perchloride of iron.

I long used cold-water injections. At p. 472 of my book is the following passage. "At one time, it was my habit to wash out the uterus with iced water first. I now prefer not to do this. . . At the period when the perchloride is especially indicated, the exhaustion is generally so great that the injection of cold water is ill borne. I am inclined to think that, under the circumstances, the injection of cold water is more hazardous than the injection of perchloride of iron."

This leads us to the consideration of Dr. Mushet's statement that "he has no dread of cold-water injections; no compunctious fears of pyæmia or metritis, or irritant influence on the uterine tissues or sinuses, as he has never witnessed any injurious effect from their employment, which cannot be affirmed of the perchloride, *if we trust the evidence adduced at a former meeting of the Obstetrical Society.*" I have italicised the concluding sentence in order to point my assertion that the "evidence adduced" cannot be trusted. Few were there who *heard* the discussion referred to, but were satisfied that the positive evidence of good done by the perchloride far outweighed the doubtful evidence against it. A report of the proceedings of a learned society is often very like a newspaper report of a trial in a court of law. One must see and hear and know the witnesses in order to judge of the value of their evidence; and, even when we have formed a just estimate of evidence, we must be careful not to build a larger structure of deduction upon it than it will bear.

I will not contend that an injection of perchloride of iron may not be followed by pyæmia. But it is one thing to show that pyæmia has followed such injection, another to prove that the injection was the cause. But, let it be granted that the injection has caused pyæmia, is the remedy thereby absolutely condemned? Is no account to be taken of the numerous cases —for numerous they now are—in which life has been saved by it? Certainly, after severe hemorrhage, under any treatment, or no treatment, the liability to pyæmia is great. That perchloride of iron adds sensibly to that liability is not proved: unless, indeed, we take the case of a woman who, but for the use of perchloride, must have died of hemorrhage. If she have pyæmia, it may truly be said that, but for the perchloride, which enabled her to survive the immediate peril of flooding, she would not have had pyæmia. But pyæmia has often occurred after the use of cold water. Will it be said that the cold water caused the pyæmia? I will not affirm that it did; but of the frequent sequence there can be no doubt. Scarcely a week passes without my having the opportunity of seeing some case of pyæmia after labour at term or abortion where iron had not been used. In some of these, experience justifies me in the belief that pyæmia would not have taken place had the iron injection been used in time to lessen the loss of blood, and thus to spare the conservative forces of the system.

I still feel justified in repeating the formula expressed at p. 474 of the work quoted.

"We have three stages of hemorrhage to deal with. 1. There is hemorrhage with active contractility of the uterus. Here the diastaltic function may be relied upon; excitants of contraction

find their application. 2. There is the stage beyond the first, when contractility is seriously impaired, or even lost. Here excitants of contraction are useless; our reliance must be upon the direct application of styptics to the bleeding surface. 3. There is the stage beyond the first two, where not only contractility, but all vital force is spent, where no remedy holds out a hope unless it be transfusion; and even this will probably be too late."

The practical directions are as follows. Take a Higginson's syringe, to which is connected a uterine tube nine or ten inches long. Mix in a deep basin four ounces of the strong liquor ferri perchloridi of the *British Pharmacopœia* with twelve ounces of water; pump through the delivery-tube two or three times to expel the air; then pass the delivery-tube into the uterus, so that its end touch the fundus of the uterus; then pump gently and slowly; the styptic fluid will thus bathe the whole inner surface of the uterus.

Had I the smallest misgiving as to the value or general safety of the practice for which I am mainly responsible, I would, without hesitation, retract all I have said, and avow my error. I have not the vain ambition to set up a "currus triumphalis" of iron. But knowing that lives dear to me, and lives dear to others, have been saved by it; and still believing that the benefits far outweigh the evils springing from it, I again commend the practice to those who have the courage to try a new remedy in desperate circumstances, and who fear to let a woman bleed to death relying too long upon means sanctioned by routine.

As I have said at the beginning, this momentous question must be decided by facts. From every quarter of the compass assurances flow in that lives have been saved by it. I earnestly appeal to every man who has had a fatal case of flooding after the use of perchloride of iron to publish as full an account of it as possible. At some future time I shall endeavour to sum up the evidence, giving faithfully the records of my own observations and reflections.—*British Medical Journal, Nov.* 29, 1873, *p.* 627.

109.—ON THE USE OF PERCHLORIDE OF IRON IN POST PARTUM HEMORRHAGE.

By Dr. Lombe Atthill, Fellow and Examiner in Midwifery, King and Queen's College of Physicians, and Obstetric Physician to the Adelaide Hospital, Dublin.

Since I commenced the practice of injecting this styptic salt into the uterus in the class of cases under consideration, I have not had a fatal case of *post-partum* hemorrhage, while I

believe I have saved several lives which were in great jeopardy. In one case only did death subsequently occur. The patient was a lady, a member of a highly strumous family, who herself, prior to the occurrence of labour, was in very bad health. Sudden and uncontrollable hemorrhage set in immediately after the birth of the child. Not a moment was to be lost, or my patient would have been beyond the reach of medical aid. What the exact strength of the solution I employed may have been I know not, for it was made by guess. I injected about four ounces of it. The hemorrhage was instantly arrested, and the patient did well for five days; then symptoms of pyæmia set in, and she finally died. I do not believe that her death was due to the injection of the perchloride; but I relate the case as it occurred. Of this I am sure—that she would have died in a few minutes from hemorrhage, had it not been restrained by the action of the perchloride. I now always carry the perchloride of iron with me, and, if any excessive loss occurs, use it as directed by Dr. Barnes in his work on "Obstetric Operations." I believe the satisfactory results which I have met with are due to my using the solution promptly, and in not waiting till the patients were in a state of collapse. For the purpose of injecting it, I use a common syphon syringe, to which an ordinary vaginal flexible nozzle is attached. This should be carried fairly up to the fundus of the uterus. I generally find that four or five ounces of the solution are sufficient. As to the strength, Dr. Barnes recommends that four ounces of the strong liquor ferri perchloridi be added to twelve of cold water. I generally use it stronger—about two parts of water to one of the liquor. One precaution only is needed, namely, to take care that the tube is passed well into the uterus before any of the fluid is injected; otherwise the vagina will be corrugated and the os uteri contracted from the effects of the iron, the styptic will not reach the interior of the uterus, and great difficulty will be experienced in any subsequent efforts to introduce the tube. The same reason, namely, the effect produced by the action of the styptic on the vagina and os uteri, renders the application of the iron by means of a sponge difficult and unsatisfactory.

In conclusion, I believe the injection of the perchloride of iron to be not only a justifiable, but, in general, a safe mode of treating severe cases of *post partum* hemorrhage, and that it, or the use of some similar styptic, can alone be relied on in severe cases.

The great injury to which steel instruments are liable, when kept in a bag which also contains a solution of the perchloride of iron, has induced me to carry the salt in the solid form, the bottle containing it being enclosed in a box-wood case.—*British Medical Journal, Nov.* 29, 1873, *p.* 628.

110.—SOME REMARKS ON POST PARTUM HEMORRHAGE.

By Dr. J. Braxton Hicks, F.R.S., Lecturer on Midwifery and the Diseases of Women, and Physician-Accoucheur to Guy's Hospital.

How can we arrest *post partum* hemorrhage?

Let us clearly understand what is the clinical condition with which we have to contend.

The most common, doubtless, is an uterus more or less uncontracted. The causes of this condition are many, but they may be resolved into two major classes; one, in which the uterus, from a long labour, or from a readily exhausted nerve-power on the part of the patient, is tired out and has used up its contractility, a state of things very frequently seen after craniotomy, &c.; the other, where the uterus has not been sufficiently excited, either the child has been withdrawn too quickly, or it has been expelled with two great ease. Any how, there has not been irritation or pressure sufficient to keep up the proper amount of reflex action after the placenta is delivered.

But, how much contraction is needed to prevent hemorrhage? Certainly, the most secure condition is the little hard uterus likened to a cricket ball. But it has not so very rarely happened to me, and from conversation I find the same has also to others, that an uterus may be anything but hard and firm, and yet no hemorrhage may result. I presume that there is a degree of contraction sufficient to arrest blood-loss, short of the hard and most desirable state. I think also much depends on the size of the sinuses patent on the inside of the uterus, a variable condition, as variable, indeed, as varicose states are. Probably, something may be owing to the state of the vascular system, both on the left side and the right side of the heart; and, again, something may be found to explain in the power of the abdomen called by Dr. M. Duncan "retentive," which, although much neutralised by the flaccidity of the walls, yet in some might neutralise the outflowing from the uterus, and this action certainly might be increased if the abdomen were bulky and pendulous, hanging over to the side. However any one, or some of them combined, may be the cause, it is certain that in some cases the uterus is considerably relaxed without the occurrence of flooding.

But, if an uterus be contracted and in an active condition, this does not necessarily ensure immunity from flooding; for, if it be distended by a piece of placenta, or a clot, or anything, or if it be prevented from well closing, as by a distended bladder, as was pointed out by the late Dr. Lumley Earle, or from adhesions, as remarked by Dr. Graily Hewitt, in any case, hemorrhage is pretty sure to arise, unless the uterus be in powerful action.

What a common case is this. A small clot, formed, perhaps, before the extrusion of the after-birth, is not expelled; the uterus, notwithstanding it is firm and hard, increases in bulk; a slight gush takes place on firm pressure, and some watery blood, or nearly clear serum, flows in small quantities from the vagina. Notwithstanding the firm and even painful contraction of the uterus, it slowly enlarges, and, upon a considerable quantity of blood escaping, the hand is introduced, and a hard firm clot is found. As soon as this is removed, the oozing ceases, and the case is safe; or should the practitioner, ignorant of the import of an increasing though hard uterus, leave the patient, he may be recalled to witness the worst terrors of flooding; for, the uterus having relaxed, all check over the oozing ceases, and the blood pours forth from every stretched out opening.

Another cause occurs at times, and gives much trouble; namely, a partial relaxation of the uterus, an irregular contraction; I mean an uterus contracting on one side only at first, then that part relaxing, and the opposite side contracting, and so on. This state permits a certain amount of bleeding, which, coagulating within the uterus and gradually added to, tends to the dangers common to the presence of clots above-mentioned. These cases cannot, I suppose, be very common. I have seen about three well-marked instances.

Another condition one finds by no means rarely, namely, the alternate relaxation and contraction of the uterus. Of course, this is the natural state of the uterus after labour, but in these the intermissions of the contractions extend to more complete relaxation. A great deal of blood may be lost this way: indeed, I have seen some fatal cases, and very many women who have been in great jeopardy by it. It is, of course, a less anxious state than that of absolute relaxation; but it is, perhaps, more deceptive.

Again, where placenta has been prævia, or near to the uterine opening, we have sometimes a very troublesome loss. For the lower zone of the uterus after labour does not contract well, indeed it is generally more or less relaxed; if on this part the placenta have been inserted, it can readily be perceived how hemorrhage can arise. But sometimes it arises after placenta prævia, though in a different way. If the bleeding from the vessels be slow, and clots become adherent to the lower uterine opening, the hemorrhage from the upper part is retained, and danger from flooding thus arises; and I may remark, incidentally, that this blocking up of the uterus, although it may not produce hemorrhage, yet retains decomposing secretions, and thus adds to the *post partum* dangers of placenta prævia.

Hemorrhage after delivery occasionally occurs from lesions of the uterine walls. The more frequent of these rarer cases is the

lateral laceration of the vaginal portion of the cervix. In these the blood is generally bright, particular in the latter variety.

Lastly, hemorrhage after delivery may occur from inversion of the uterus, partial or complete. If incomplete, the cause may be overlooked. I was a year or two since asked to see a case of flooding, and to bring my transfusion apparatus. The patient was dead before I arrived; I, however, passed my hand to ascertain the cause, and found the upper half of the uterus inverted, feeling like a polypus just within the os. I had no trouble in reducing it.

If these causes be duly borne in mind, the principle of treatment is clear enough. If the uterus be inert, we must induce it to contract by the various plans well known to all; if we cannot do this, we must do what we can to coagulate the blood and to cause a contraction of the inner surface of the uterus by styptics locally applied. If a clot of blood, or fragment of placenta, be the cause, then its removal, either by external pressure or introduction of the hand, or by these two plans together, is the clear indication. If irregular action be present then our attempts must be directed to ensure a more perfect action.

If lesion be present in the body of the uterus, it is clear it would be highly dangerous to use any styptics if the laceration extended to the peritoneum; but, if the hemorrhage came from the vaginal portion of the cervix, if cold and pressure failed to stop it, a sponge dipped in styptic and firmly pressed on the rent would be the most sure way of arresting it.

But the categorical account in many text-books of the various modes of checking *post partum* hemorrhage fails to convey the clinical assistance required in the case; because, in most cases, either from the suddenness of the attack or from carelessness of observation, the practitioner is brought face to face with serious loss, and action must be prompt. But is everyone prepared to meet this possibility? How do many go to a case of midwifery. Of course, if, in one's rounds, one be called to a case, complete provision is not to be expected; but, when called straight from home, how few take anything with them. Thus, if the bladder be full, there is no catheter; if a languid uterus suggest the possibility of subsequent inertia, there is no secale; and if hemorrhage actually occur, then the practitioner has to meet it only with cold water and his hands. But, if we thus undertake a case, are we much better than the simple woman-midwife, who is not allowed to use secale, and has nothing besides? No one should go to a case without a catheter, secale, opium, ammonia, a good syringe, and (as far as my present experience has shown me its value) the perchloride (or a kindred preparation) of iron.

When, then, the placenta is delivered, a careful practitioner, instead of leaving the bedside, will take charge of the uterus, either by his own hand or by closely watching his assistant; and also will either keep the vulva in view, which can be done without exposure to any extent, or will frequently examine the napkins; and, if he finds the uterus relaxed in part or wholly so, if it be still enlarging, notwithstanding its firmness, or if he see hemorrhage arise, he is instantly ready to act. He will, however, have already unobservedly prepared measures in anticipation—cold water in plenty, some already poured out in a basin; and broken ice if obtainable; with the syringe charged ready for instant service. He will have some fluid preparation of secale at hand. If he possess only powder, then he will have already made a decoction. It may be that in ninety-nine per cent. of cases these arrangements will not be wanted. So much the better for the ninety-nine, but so much the safer for the remaining one to have them ready.

But, besides these precautions in ordinary cases, there are some which should be adopted in cases where we suspect, as the case approaches termination, that hemorrhage may arise. I mean after very long labour, much exhaustion, a languid action of uterus, where the interval between the pains is long, the pain short, but the progress during it rapid. In these cases, it is always better to give secale just before the head comes on the perinæum; and, where the child glides out rapidly with slight pains, it is better to carefully check the descent of the head, so as to rouse the uterus by an impediment. It is also better in these cases not to hurry away the placenta. Its presence acts as a stimulus to the contracting power of the uterus; and, if the uterus be not expanding, nor hemorrhage occurring, no reason exists for its removal. But of course the medical attendant must not leave the uterus to any one; it must be under his grasp the whole time; and specially he must not drag on the funis, but be ready, as soon as hemorrhage appears and external grasping fails to extrude the placenta, to pass the hand boldly in and remove it. I think that, among the causes of after-relaxation of the uterus, is the removal of the placenta from the hesitating uterus. Of course, if the hand in removal be passed rather roughly, and the internal surface excited by the free touching, it compensates somewhat for the removal; otherwise it, like a premature removal of the fœtus, is apt to leave a flaccid uterus.

Supposing, then, that after the placenta has been delivered, and we have left the uterus in charge of the nurse's hands, or after the application of the binder (which latter has the great fault of concealing rather than revealing) we find the patient pale, and the uterus largely distended, we should

instantly pass the hand on to the uterus firmly and quickly, and press it till, the blood having escaped, it is sufficiently small enough to be grasped, which should be done firmly, accompanied by friction of its surface. I always pass the hand into the vagina, and remove the clots; and, if the flooding have ceased, I merely keep up the grasping and friction on the uterus; but, if any hesitation to contraction be present, I apply cold, externally, by the hand or napkin dipped in cold water; at the same time I inject cold water into the vagina. Should, however, these plans fail, or the hemorrhage be profuse, I pass at once the hand within the uterus, and remove every clot, examining the whole inner surface to see if any portion of placenta or membrane have been left attached. It seldom occurs but that the uterus contracts and remains so, but should it relax, I repeat the clearing out (for clots are almost sure to be there), and carry the syringe jet to the fundus and throw in cold water; or, if ice be at command, I carry in a lump and rub the inside of the uterus all over with it; this is generally a most efficient remedy. If the uterus fail to contract or to remain contracted, the condition of the case is very urgent. The hand should be carried in again, and the uterus well titillated; of course, during the whole of this action the exterior is to be firmly grasped. Unfortunately, the blood-loss has lessened the chance of contraction; but, fortunately in very many cases at this crisis, syncope steps in, time is given for coagulation in the uterine vessels, and the patient is saved. It is generally those patients who never faint that die, if we cannot stop the flooding. Only two measures in this desperate state of things, remain to us; the first of which is pressure upon the aorta and vena cava. A diaper can be folded hastily into a firm pad, and pressed on the centre of the abdomen, about the navel; the patient, if not already on her back, should be so placed. I would recommend that the legs be not elevated, because when the return current of the vena cava to the heart is stopped, if the legs were lifted, the blood would regurgitate to the uterus, and still more be lost. I think this plan should be more frequently adopted than it is. Like fainting, it permits coagulation.

Hitherto, I have been considering the more severe kinds of flooding, where we have no time to temporise, no time for secale, galvanism, and emetics. But in all cases of flooding, where grasping and friction externally, coupled with cold injections into the vagina of cold water, and secale, fail to produce cessation of the flooding, the best and most secure plan is to pass the hand into the uterus, and remove anything that there may be found distending or tending so to do; cold can then be carried within the uterus by syringe or ice, when in most cases the

bleeding ceases. If, however, we find a clot with an active uterus, we may spare the patient the passing the hand within the uterus, if we break up the clot as far as two fingers can reach, and employ pressure from without. If, however, the bleeding continue, it is shortest to pass the hand inside the uterus, and remove anything which may keep up the bleeding; and if we find no fragment of placenta or adherent clot, we should examine for a lesion. If there be none, and the bleeding still continue, after the application of cold, particularly if there be partial or intermittent relaxation, then we must try aortic pressure; and this failing, our only resource is to apply a styptic to the interior of the uterus. But I see that a writer in the Journal, in antagonising the use of perchloride of iron, says that, with one or the other plan, we can secure contraction, and therefore have no need of a styptic. I must say this remark fills me with astonishment; and I think it must also others, quite as alive as I am, and as he is, to the importance of establishing reflex action; and using all the remedies as sedulously as he recommends. Like Dr. Barnes, whose area of work embraced much of the same classes as my own, I hailed with a feeling of, I may say, extreme satisfaction, a plan which would give some hope of help, when every other attempt to establish uterine contraction had failed.

In my own practice, I employ a rather weak solution at first —one of the weaker liquor ferri perchloridi to six or eight of water. If that fail, then I make it stronger; but, if the flooding be rapid, I use the full strength of the weaker solution or the tincture, because much of it is lost in the blood, or is prevented from coming easily into contact with the walls. The tube of the syringe should be carried to the fundus, and the fluid gently injected; or it may be conveyed with a sponge, which should be pased over all the internal surface. Perhaps this is the safest mode; but it must be removed promptly, or the uterus may retain it, as occurred in one case I know. I have tried it in all kinds of *post partum* hemorrhage, after removal of clots, which is important, or they become coagulated and firm, and cannot be removed afterwards without trouble. I have found it particularly satisfactory in cases of irregular and intermittent relaxation, causing the uterus to assume its proper form at once.

I do not enter into the discussion as to how it acts. It stops blood in hemorrhage from a wound on the outside; I presume it acts similarly on the inner side of the uterus—of course not requiring general uterine contraction. I have no doubt, in the less inert conditions, it rouses the remnants of contraction, and acts as cold and the presence of the hand. In conclusion, I will add that, till any one can show a better remedy, or till I

find it of greater disadvantage than extreme loss or death, I shall continue to use it in the fullest confidence, or till it has been clearly pointed out that it is dangerous to use.—*British Medical Journal, Jan.* 17, 1874, *p.* 74.

111.—THE TREATMENT OF POST PARTUM HEMORRHAGE.

By Dr. D. M. WILLIAMS, Liverpool.

I have had eighteen years' extensive midwifery practice, during which time I have attended about four thousand cases—yet *I never lost one* from *post partum* hemorrhage; never had occasion to introduce ice into the vagina, nor to inject cold water. I remember only one case in which I was compelled to introduce my hand into the uterus to check hemorrhage. The means I have employed are the usual ones already referred to by other correspondents, with two trifling additions. 1. After the birth and separation of the child, I grasp the fundus uteri, turn the patient on her back, and, with the first pain, generally succeed in expelling the placenta, and thus completing the labour in from five to ten minutes' time. Then I make the patient comfortable, remove all wet clothes, and put on a tight calico binder next the skin. 2. If hemorrhage occur, and remain unchecked by the usual means, I flex the leg (nearest the edge of the bed) on the thigh, get the bed-clothes slightly raised, and fan the patient freely with cold air. She feels the air colder than the water, for obvious reasons, and the hemorrhage is soon checked.—*Brit. Med. Journal, March* 21, 1874, *p.* 379.

112.—SOME REMARKS ON UTERINE HEMORRHAGE DURING THE PUERPERAL PERIOD, ESPECIALLY WITH REFERENCE TO THE TREATMENT BY THE INJECTION OF STRONG STYPTICS.

By Dr. T. SNOW BECK, F.R.S., Member of the Royal College of Physicians.

[The injection of strong styptics into the cavity of the uterus, with the object of arresting hemorrhage after parturition or abortion, has of late attracted considerable attention. Dr. Hugh Norris, of South Petherton, was one of the first, in this country, who directed attention to the use of these styptics. Dr. Norris remarks: "The injection was only recommended in cases of urgency and peril." The opinions of other writers on the effect produced by the injection on the uterus may be gathered from the following quotations:]

"In a few minutes the uterus, and especially the os, was firmly

contracted" (Dr. A. B. Steele).—"In two minutes the uterus was felt gradually contracting around the hand; with the contraction the bleeding ceased, and the coagula passed out of the uterus with the hand as the uterus contracted. In five minutes the uterus had freely contracted" (Dr. T. Chambers).—"As I injected I felt the uterus contracting around my hand, and not a drop more blood was lost" (Dr. W. S. Playfair).

In cases where a fatal result has followed the injection of the perchloride of iron into the cavity of the gravid uterus, and where an opportunity has been afforded of examining the uterus after death, it was evident that only partial contraction had followed the use of the styptic. Sufficient contraction had been induced to arrest the flow of blood from the arteries; but the veins or sinuses remained open and pervious, and were found to contain an ink-black coloured fluid, which gave abundant evidence of iron by the usual chemical tests.

Although this means of inducing contraction of the uterus, and thus arresting post-partum hemorrhage, was at first recommended and used as a "dernier ressort" in cases of great peril, and where other remedies were said to have been employed and had failed, yet of late it has been employed in all cases of post-partum hemorrhage to the exclusion of all other remedies. It has also been employed to prevent the occurrence of hemorrhage which had been anticipated from the experience of former labours in the same individual. And so far as we at present know, no reasonable objection can be urged to the employment of these strong styptics; provided the one essential condition be attained—the complete and permanent contraction of the gravid uterus. For it appears a matter of little consequence by what means the contraction is produced; so long as it be complete and permanent, as to close the arteries, and obliterate the canals of the veins or sinuses, and prevent any injurious absorption, the woman is safe, and will make a good recovery. But the danger in the use of these agents, as shown by the fatal cases which have followed the employment of them, appears to consist in only partial contraction being produced, by which although the hemorrhage was arrested, yet the veins or sinuses remained open, and permitted the perchloride of iron to be taken up and conveyed into the general system. The consequence of this, as shown by the fatal cases, has been that symptoms of blood-poisoning supervened in the course of a few days, ending in the almost inevitable death of the individual. For according to our present knowledge, once these substances have been introduced into the general system to any amount, there are no means by which the fatal effect can be arrested. In some cases no perceptible contraction has followed the injection of even a strong solution of the per-

chloride of iron; and in others the uterus has again relaxed. Hence arises the question, Are we justified in the employment of these substances, which may fail to produce contraction, or, in other cases, complete contraction, and in failing be followed by such deadly results, when other means may be employed, not followed by such serious consequences, and by which the same end may be attained—the complete and permanent contraction of the gravid uterus?

I have endeavoured to show that when the means in ordinary use, as they are termed, are efficiently and properly used, complete and permanent contraction of the gravid uterus may be effected, the hemorrhage arrested, and all injurious absorption prevented.

As an illustration of the manner in which the usual means of inducing contraction are frequently employed I may quote the the following case from several similarly treated which have come under my notice.

Mrs. D., aged forty years, a Mexican by birth, of small stature, dark hair, and dark complexion was confined of her seventh child in September, 1870. All her previous labours had been natural except the second, at which the perineum had been ruptured, and extended into the rectum. She had not any control over the rectum, yet had been living with her husband in this state, and having children. She was very well in general health, only could not eat anything; was low, weak, and very low spirited; and so persuaded she would not survive her approaching confinement that she made all arrangements for her decease. Lingering pains commenced about 5 a.m., and at 10 a.m. the physician accoucheur who had engaged to attend her was sent for. On his arrival, he said all was going on well, and ordered her to take champagne and some food; the latter of which, however, she could not take. At twelve o'clock the physician accoucheur returned, and in consequence, he said, of finding her so low, administered chloroform, applied the forceps, and at 1 p.m. extracted the child apparently dead, but in about half an hour it recovered. The lady remained quiet, being still under the influence of the chloroform, and at the end of half an hour the afterbirth was removed. About ten minutes after the removal of the afterbirth flooding of a bright red blood began in torrents—"it simply poured out." Ice was freely passed into the vagina until it would not admit any more; all the clothes were taken off the bed, and ice applied to the outside of the abdomen; the doors and windows were opened; and another physician accoucheur was sent for. After his arrival the galvanic battery was used, but without any effect. The body was now stripped naked, and water poured upon the abdomen from a height until the bed and the room

were swimming with blood and water. Quantities of brandy were given as long as she could swallow, and when she no longer could do so, brandy and ergot of rye were twice injected into the stomach. But the hand of the accoucheur was never passed into the uterus after the hemorrhage had commenced; nor was there any pressure at any time applied to the uterus from the outside of the abdomen; when about 3 p.m. the lady was declared dead, but continued to gasp at intervals until 4 p.m.

The most strenuous advocates for the use of styptic injections do not deny that fatal cases have occurred after the injection of the perchloride of iron to suppress post-partum hemorrhage, but every possible excuse has been put forward to palliate these deaths when they have taken place. It is a frequent excuse to say that death arose from "pyæmia" or "septicemia," which might arise from various causes; at other times it is said to have been caused by "puerperal fever," or "puerperal peritonitis," and "other grave complications;" it has also been attributed to "shock," or to any other cause the circumstances of the case would admit of. I have already recorded one case, with a somewhat detailed account of the appearance presented by the uterus and appendages, where the death could not reasonably be attributed to any other cause than the absorption of the perchloride, and consequent blood-poisoning, and I may now add two other cases.

Mrs. A. H., aged twenty-eight years, was confined of her third child on August 25th, 1870. The confinement was perfectly natural, the afterbirth large, but readily removed, about 1 p.m., by the physician accoucheur in attendance, and accompanied by the loss of a great deal of blood. A bandage was applied round the abdomen, and all considered perfectly satisfactory. About 3 p.m. the nurse observed the lady looking pale; apparently sleepy; and then noticed a stream of bright red-looking blood trickling from the bed to the floor. She directly called the physician accoucheur, who was still in the house, and who endeavoured to arrest the hemorrhage, by introducing into the vagina three pieces of ice successively, each about the size of a goose's egg, and flapping the abdomen with wet cloths. This treatment was continued about fifteen or twenty minutes; but no pressure was at any time applied to the uterus. The lady was becoming exhausted, and the respiration sighing; when, at the end of the time named, the uterus was injected with the perchloride of iron. The hemorrhage was stopped at once, and never returned. Brandy and champagne were very freely administered. The lady soon recovered, and was pretty well for two days and a half, sitting up in bed, and making doll's

clothes for her children. The perchloride of iron was injected only once; and what afterwards came away from the womb was of a dark reddish-brown colour, destroying everything. No coagula passed. The lady was very well on the night of the second day; but about 8 p.m. of the third day, or about sixty-five hours after the injection, she called the nurse and simply said she was dying and sinking fast, and requested the physician to be sent for directly. As the lady did not appear to be ill, they tried to delay sending, but finally did so. The physician accoucheur came, but did nothing, saying there was a clot in the uterus to come away; but none came. There was not any pain complained of. In the afternoon of the same day the abdomen began to swell, attended with pain, and hot flannels were applied. The swelling of the abdomen and pain increased, with great flatulency. A long elastic tube was passed into the bowels from time to time, to remove the flatulency, when much wind passed, and gave great relief for a time. Each time this tube was introduced, a thin, quite black fluid escaped with the wind; sometimes as much as half a chamber-pot full at one time. The abdomen kept gradually swelling; then both arms gradually swelled to as large as the thighs; the hands being also puffed up. The legs swelled, but never to much extent. She could not move the arms, nor herself in bed. The swelling kept gradually increasing during six weeks. She was incessantly talking; fearfully irritable; and screaming to be moved every few minutes, day and night, in consequence of the severe pain after being a short time in one position. She could not move any part of the body; slept very little; took very little nourishment; but quantities of brandy and champagne. Very little lochia, or any discharge, passed from the uterus, except some pieces of membrane at the early part of the illness. Up to the last fortnight she knew the nurse and her children when her attention was forcibly directed to them; but during the last fortnight she knew very few around her; though still dreadfully irritable, and desirous to be moved every few minutes. Towards the end she lay in a muttering, talkative delirium, but knew no one. During the last twenty-fours the swelling extended to the throat, and she died in her sleep, without a word, on October 4th, 1870; or the fortieth day after her confinement and the injection of the perchloride of iron.

This case, which was also conducted by a professor of midwifery in one of the largest medical schools, presents most, if not all, of the objectionable points in treatment noticed in the preceding case. It was also one of a numerous class of cases where post-partum hemorrhage takes place; cases where the uterus contracts sufficiently to expel the child and throw

off the placenta, yet afterwards gradually relaxes, and within a space of a few minutes, is so far relaxed as to allow the blood to escape from the torn utero-placental arteries. A healthy young lady was confined with a perfectly natural labour, though accompanied by a considerable loss of blood, which is usually indicative of a want of efficient contraction of the uterus. No precaution was taken, beyond the application of a binder, either to maintain the contraction which had taken place, to render it more complete, or to prevent the organ becoming relaxed. About two hours afterwards a stream of bright red-looking blood was noticed trickling down to the floor. The professor was immediately recalled upstairs, and endeavoured to arrest the hemorrhage by introducing three pieces of ice into the vagina and flapping the abdomen with wet cloths. Had I the least doubt as to the accuracy of this account, I should question whether any one in the position of this gentleman could be so ignorant of what was required to be done as to pursue such practice. Brandy and champagne were freely administered, which, by stimulating the action of the heart, would only enable it to force out more blood, to augment and maintain the hemorrhage. As might be expected from such treatment, the hemorrhage continued, and in about fifteen or twenty minutes symptoms of exhaustion, with sighing respiration appeared. As in other cases, the usual formula would, without doubt, be reiterated, that the remedies in ordinary use had been diligently applied, and had failed—whilst in point of fact nothing which ought to have been done had been done, and much which ought not to have been done, as the free administration of brandy, had been done. At this juncture a solution of the perchloride of iron was at once injected; the hemorrhage was directly arrested, and all appeared *couleur de rose* for two days and a half. At the end of this short time an inward feeling told her she was dying and sinking fast, though no appearance of illness was observable to those around her. It does not appear why a clot should have been supposed to exist in the uterus; none, however, came away, nor was any pain complained of. In the afternoon of the same day symptoms of blood-poisoning commenced, the abdomen began to swell, attended with pain. The swelling and pain increased to such an extent that it was considered desirable to introduce a long elastic tube into the colon to relieve the distension. Quantities of brandy were given. The arms gradually swelled up until they became as large as the thighs, the swelling finally extended to the throat, and the lady remainded for six weeks in a distressing state. She took little nourishment; was incessantly talking; fearfully irritable; incapable of moving herself in bed; and screaming every few minutes, night and

day, for her position to be altered in order to gain a little relief from the change of position. For some time she knew those around her when her attention was roused; but during the last fortnight she knew very few, lying in a muttering talkative delirium, and finally passed quietly away in her sleep.

No post-mortem examination was wished for, and none was made. Yet there can be no reasonable doubt, from the experience of other cases where an examination of the body was permitted, that the general system was poisoned by the absorption of some of the perchloride of iron which had been injected into the relaxed gravid uterus. And though the symptoms which were developed greatly resembled those which attend pyemia or septicemia, yet there were destructive characters which render it evident they were not produced by the same cause. Swelling of the arms has been noticed in other fatal cases after the perchloride of iron had been injected; also the escape of thin ink-black colour fluid from the bowels. But the quantities which came away in this case were unusually large, and so far as I know, this peculiar black fluid has not been noticed except in those fatal cases which have followed the injection of the perchloride of iron.

Dr. Graily Hewitt, during the discussion at the Obstetrical Society, referred to "one case, in his own practice, where the perchloride of iron injected had been used to restrain hemorrhage, and the patient had subsequently died," and remarked, "after three days pain set in, the lochia became arrested, and the patient died from puerperal peritonitis and other grave complications five weeks after delivery. Whether this result was in any way due to the action of the iron was a question." (Obstetrical Journal, April, p. 45). But where are there any evidences of "puerperal peritonitis," in such cases as these? What "other grave complications" could so suddenly spring up in a healthy young woman, if not those consequent upon her confinement? Can there be a reasonable doubt that the death in this case was caused by the perchloride of iron which had been injected into the uterus, which, whilst it arrested the hemorrhage, was afterwards taken up into the system, and caused the death of the individual more certainly than the hemorrhage it stopped? Had means been adopted to insure and maintain perfect contraction in this case immediately after the use of the styptic, it is most probable the fatal hemorrhage might have been prevented; but the danger of employing these substances is still very apparent.

Reasoning upon the experience obtained by the injection of the perchloride of iron, and considering that the chief danger appeared to arise from the serious effects of this substance

when conveyed into the general system, the question was naturally suggested, whether some stimulating astringent could not be thought of which would excite the uterus to contraction when injected into the cavity, but which would not be followed by such serious consequences supposing a portion did become taken up into the general system. In large towns it is generally possible to procure a supply of ice at all seasons of the year; but in the country it appeared desirable to have some agent which could be generally obtained, and which might be resorted to as an additional means of inducing contraction. It occurred to me that ordinary vinegar might fulfil these indications; and with this view I have employed it on some six or eight occasions, in the proportion of one part of vinegar to two or three parts of water, with very good effect and encouraging results. For example:—In a case of prolonged labour with the third child, which I saw after the young woman had been in labour many hours; where the uterus was acting strongly, and the "woman quite worn out;" the forceps was employed in consequence of the large size of the child's head, and the relative small capacity of the pelvis. The placenta was thrown off by the uterine contractions; but after removal of it from the vagina, considerable, but not serious, hemorrhage followed, which was not arrested by the injection of cold water, pressure with the hand from the abdomen, and two doses of ergot of rye. One part of vinegar, with two parts of water, was then injected into the uterus, and the hemorrhage was at once completely arrested. And so complete was the uterine contraction induced, that the nurse affirmed there was none of the usual lochial or red discharge afterwards. Certainly none was apparent at the subsequent visits. In an abortion between the third and fourth month, where considerable hemorrhage had been going on for more than four hours, and which had not been influenced by the remedies employed, one part of vinegar with three parts of water were injected into the cavity of the uterus. The hemorrhage was at once arrested, and the woman had an excellent convalescence. In this case also the usual lochial or red discharge was again absent; and the same result has been observed in other cases.

I do not mean to say that this is the most suitable agent which can be thought of as an additional means of inducing contraction; for it is not improbable that when attention is directed to the point, a more certain and more efficient means may be found out. But it certainly has acted very well so far; and it is not probable it will be followed by the same serious consequences as the solution of perchloride of iron. I would also suggest the desirability of using a weaker solution with cold water after every labour, in conjunction with the

administration of the ergot of rye, to induce complete and permanent contraction of the gravid uterus; and thus to obviate, as far as possible, those distressing puerperal complications which too frequently follow childbirth.

[Dr. Snow Beck is evidently not aware that vinegar has been used some time since in the way he here mentions. The late Professor Davis, in his admirable work on Obstetric Medicine published in 1836, p. 1065, writes:—"The principal chemical irritant usually employed for the subduction of uterine hemorrhage after delivery is an injection made with one part of vinegar and two of water conveyed into the uterus with a syringe, which, according to the testimony of some of the most respectable members of the profession in our own country, must be possessed of considerable power to suppress floodings."—Ed. O. J.]—*Obstetrical Journal, Feb.* 1874, *p.* 712.

113.—CASES WHERE DEATH HAS FOLLOWED THE INJECTION OF THE PERCHLORIDE OF IRON INTO THE UTERUS TO ARREST POST PARTUM HEMORRHAGE.

By Dr. T. Snow Beck, F.R.S.

Case.—Mrs. J. H., aged 23, of rather small stature, of fair complexion, blue eyes, and light hair, was taken with lingering pains on the evening of May 19th, 1872. Her husband had left her some time previously. She was very desponding; did not care what became of her; complained of a general feeling of lassitude; had pains in the limbs, headache, and no appetite. Her urine was thick, high coloured, and small in quantity. The pains continued much the same till the evening of the 21st, when they became stronger, and the orifice was dilated to the size of a florin. She slept pretty well during the night. In the morning the pains increased, and the membranes broke; after this the pains became strong and rapid, and the child was born about noon on the 23rd. The vagina was hot and tender. It was necessary to introduce the catheter. A good deal of blood was lost, and the placenta did not come away; after waiting about half an hour, the hand was introduced; the orifice was not contracted, and the placenta was situated at the upper part of the cavity; the uterus did not contract strongly in the hand, and some blood was lost in withdrawing the placenta. The pulse was very quick, about 130. A binder was applied. After about half-an-hour, the patient was observed looking very pale; and, on inquiry, said "something warm seemed to be running away." On examination, considerable hemorrhage of a bright red colour was taking place from the vagina. Firm pressure was now made by grasping

the uterus; cloths dipped in cold water were applied to the vulva, and flapped on the abdomen; ice could not be procured. The hemorrhage lessened, but still continued; the patient became very faint, and the respiration sighing; cold water was now injected into the uterus. As the blood still continued to flow, about half-a-pint of solution of perchloride of iron, one part of strong solution to six parts of water, was injected into the cavity of the uterus. The hemorrhage was very soon stopped; the faintness passed away, and the binder was reapplied. Everything seemed doing very well until the evening of the 26th, the third day, when she complained of feeling very ill; an extra blanket was put on the bed; a dose of chloric ether, compound spirit of ammonia, and lavender was given. The patient became warmer; perspired freely, and slightly delirious in the night. About 4 a.m., a very black clot came away.

In the afternoon of the 27th, the fourth day, the pulse was very small, and so quick it could not be counted. She lay on the back, with the knees drawn up. The abdomen was full and tender. Light nourishment, with milk, was given; warm fomentations, with turpentine, were applied to the abdomen; and an enema with castor-oil and turpentine was administered.

On the 28th, she was much better. The abdomen was not so distended; the enema brought away much flatus. On the two following days she was not so well.

I saw her on May 31st, the eighth day. She was lying on the back, with the knees drawn up; the face was slightly flushed, the expression anxious. The eyes were hollow, and surrounded by dark areolæ. She was quite conscious, but, the nurse said, wandered and talked all night. There was slight pain at the top of the forehead; the skin was warm and dry; the pulse small, 140, slightly wiry; the abdomen much distended, smooth, tympanitic; there was considerable tenderness all over, but more so at the hypogastrium and left iliac region. She could not extend the legs, on account of the pain, nor turn on either side. The lips were dry; the tongue was coated with a thin yellow fur. She was very thirsty; had no sickness; the bowels were moved twice, the stools being loose, very black, but not offensive; much flatus passed from the bowels, and the lochia were free and of a dark brown colour. She was taking light nourishment—beef-tea and milk. Five grains of carbonate of ammonia were given in infusion of calumba every four hours; and turpentine stupes were applied to the abdomen.

I saw her again on June 3rd, the eleventh day. She had been getting worse. She was lying on her back, with the shoulders low and the knees drawn up. Respiration quick, 42

in the minute; pulse 135, very small; skin warm, dry; face slightly flushed. She did not notice anything, yet could be roused; apparently dosing, muttering, and talking, occasionally giving a deep sigh and crying out. She took any drink when offered, but did not ask for anything. There was no sickness. The bowels were moved into the bed, the stools being fluid, and very black. The abdomen was greatly distended; it was apparently tender, as she cried out when it was pressed. The upper part of both arms was swollen, and appeared to be tender when pressed.

She continued in much the same state, becoming more difficult to rouse, appearing to lie in a dosing, muttering, delirious state, occasionally crying out, and quietly died on the morning of June 7th, the fifteenth day after her confinement.

The body was examined on the afternoon of the following day, June 8th. The weather was rather warm. There were no signs of decomposition. The abdomen was much distended, tympanitic; the arms were swollen, and the veins near the chest full. The intestines were much distended with flatus, especially the small intestines; their coats were thin, not injected. The stomach was apparently natural. Only a little brownish-yellow fluid was found in the abdomen; there was no exudation of lymph anywhere in the peritoneum. The uterus lay in the pelvis; it was removed for examination. The blood which flowed from the pelvic veins when divided was of a deep brown colour. The lungs, heart, liver, and kidneys were all apparently healthy, but were only superficially examined.

The uterus was large and doughy; it measured nearly 5 inches long by $4\frac{1}{4}$ inches broad at the fundus. There was some slight leaden marking at each side; the veins were not much congested. The Fallopian tubes and ovaries were apparently healthy. On cutting through the posterior walls of the uterus, different sinuses were divided, which contained a thin, opaque, ink-blackish fluid. The inner surface of the organ was covered with dark brown mucous secretion; the tissue beneath was of a deep reddish brown colour mottled with black. When washed with a stream of water, the surface was uniform, with the usual reticulated appearances of the inner portion of the contractile tissue. Studded over the previous situation of the placenta were numerous larger and smaller round black spots, which proved to be the orifices of the sinuses, containing a similar opaque ink-blackish fluid. Incisions made into the substance of the walls, at and near the seat of the placenta, divided several sinuses, all containing more or less of a similar opaque ink-blackish fluid. No remains of any coagula were anywhere observed. Some of the arteries

were picked up with the fine point of a needle, and were found to be open, free, and without any appearance of coagula in their canals.

Case.—In the afternoon of October 2nd, 1872, I received a small box, by railway, from a medical friend in Yorkshire, containing the uterus and appendages of a patient, and accompanied with the following history.

"The wife of a farmer in this neighbourhood was confined of her fourth child on September 7th, 1872. She was a healthy woman, and had been quite well up to the time of her last confinement. The labour was lingering; and, as I had been with her a long time, I applied the forceps and completed the delivery. As the placenta did not come away, I introduced my hand and took it. I applied a binder as usual, and all appeared going on nicely. I was just leaving the house, when the nurse called me and told me my patient was flooding, and I found a good deal of bright red blood running away. Mrs. W. was pale, but not faint; I passed my hand into the uterus and removed the clots. I then gave a full dose of ergot, applied cold water externally, and, after a time, injected cold water into the uterus, but the flooding still went on. After a time, I made the nurse press upon the uterus, and mixed a strong solution of the perchloride of iron, which I had with me, with six or eight parts of water, and passing a large male catheter into the uterus, injected half-a-pint or more of the iron mixture. The flooding soon stopped. I applied the binder again, gave a drachm of the fluid extract of ergot, with some brandy and water, and left. All went on very well till the third day, when Mrs. W. said she did not feel at all well. She did not look bad; but the abdomen was full and tender when pressed, and the pulse very quick. She took a dose of castor-oil, and I sent her some carbonate of ammonia and chloric ether, in a bitter infusion, to be taken three times a day. The next day she was better; the day after, not so well. Then the abdomen became more swollen, and was very tender; the pulse very quick and small. She was very thirsty; had no sickness; the bowels were loose, moved twice a day, and very dark coloured; and a great deal of talking, and wandering during the night. She took her food very well; but lay on her back, with the knees drawn up, in a dreamy state, mumbling and wandering. She gradually became worse and worse, until she knew no one, and died quietly on September 28th, the twenty-first day after her confinement."

The uterus was large and doughy; it measured full 5 inches long, by $4\frac{3}{4}$ across. There was no exudation of any kind on the peritoneal coat; the veins were a good deal congested; the Fallopian tubes were a good deal injected, but otherwise

healthy, as were also the ovaries. On cutting through the posterior wall, different sinuses were divided, which contained some thinnish black-looking fluid in their cavities. Some dark red brown coloured fluid was found on the inner surface, which, when washed away, left the tissues of a dark red brown colour, mottled with ink-black. The seat of the placenta, and the space round it, were spotted over with black spots, which were the openings of the sinuses, filled with a similar black fluid. Incisions into the walls divided several sinuses containing a similar black-coloured fluid. No coagula were found anywhere, so far as the examination was made. The arteries were not examined.

It is singular how these cases are a mere repetition of the same facts, with only slight modification; but this probably arises from their being a repetition of a natural process occurring in healthy women, and not being the result of any previous disease. After the completion of a natural labour, the uterus becomes sufficiently relaxed to allow the blood to escape from the open ends of the torn arteries. The means which are adopted to induce the contractile tissue to again contract and close the canals of these vessels, either fail to provoke contraction, or only induce partial contraction; and in either case the hemorrhage continues, when a solution of the perchloride of iron, of varying strength, is injected into the cavity of the uterus. After the injection of the perchloride, the hemorrhage ceases at once, or very soon afterwards; the patient revives; and all things appear to be going on favourably till about the third day, when the usual series of fatal symptoms commence by the woman complaining of feeling very ill. The symptoms, once begun, continue gradually to increase in gravity, and at the end of two, three, or four weeks, the patient appears to fall asleep as death closes the scene. In the examination of the body afterwards, all the organs are found in a healthy condition; the intestines are greatly distended with flatus; there is little injection of the peritoneum, or effusion into the peritoneal cavity; the uterus is found large, and the uterine sinuses more or less filled with an ink-black coloured fluid, which, from its similarity to the secretion on the inner surface, leaves no doubt that some of this fluid has entered these canals through the open orifices at the inner surface of the uterus; and, as fluids are known by experiment to pass readily from the vena cava along the uterine sinuses, and to escape at the inner surface of the organ, it appears to admit of no reasonable doubt that the fluids at the inner surface have followed a contrary course, have entered the open orifices of the sinuses, been conveyed into the general system, produced blood-poisoning and death; all the other uterine tissues being found in a perfectly healthy state.

A similar sequence of events, and similar *post mortem* appearances, are known to follow in cases of puerperal septicæmia—so long known under the term of puerperal fever—the only difference between the two conditions being in the nature of the fluid taken up from the inner surface. In septicæmia, it is evidently the natural secretions formed at the inner surface of the uterus which are found in the sinuses. "On cutting through the uterus at the side of the placenta, a large quantity of puriform matter flowed out, as though an abscess had been laid open; this followed each section." (Obstetrical Journal, p. 443.) After the injection of the perchloride of iron, it is generally the secretion at the inner surface, mixed with the perchloride, which is found in the sinuses; and it is not improbable that some of the symptoms observed in cases where death has followed the injection of the perchloride, are the effects from these noxious secretions, modified and rendered more fatal by the addition of the perchloride of iron which had been injected. I do not know of any instance where the perchloride of iron has been probably taken up without being mixed with the secretions from the inner surface, except in the case which occurred in Dr. Barnes's own practice, and which no doubt his modesty prevented him recording, where the injection, in a woman of strong wiry constitution, was directly followed by severe pain in the hypogastrium, and death in about seven hours. The deadly effects of the perchloride, when introduced into the general circulation, are also shown in those cases where a few drops have been injected into a nævus in infants, and death has followed in a few minutes. In one case, the "*post mortem* examination showed that the point of the syringe had penetrated the transverse facial vein, and that the blood in the right cavities of the heart had been immediately coagulated."

The whole mischief in these cases of *post partum* hemorrhage appears to turn upon the single point of the want of proper contraction in the gravid uterus; and the sole question in the treatment upon the best means of inducing and maintaining complete contraction. Want of contraction, or subsequent relaxation, which practically is the same thing, allows the blood to escape from the arteries; want of proper contraction permits the canals of the sinuses to remain pervious, the noxious fluids to enter them, and death to result. Hence the effect which immediately follows the injection of the perchloride into the uterine cavity becomes of considerable importance. In the previous cases, all the tissues of the uterus were perfectly healthy; there was not the least appearance of any local action, beyond some of the tissues being stained of an ink-black colour. The perchloride is known to be a powerful stimulant to the contraction tissue, and to induce permanent con-

traction. And I conclude that the good effect which follows the use of it arises from the effective contraction which is produced; for, when complete and permanent contraction is induced, the woman appears to be safe from all ill consequences, and will make a good recovery. But it is also known that even the use of the perchloride is sometimes not followed by any perceptible contraction; or by only partial contraction, by which the flow of blood is arrested, and the way to injurious impregnation of the general system left open.—*British Medical Journal March* 7, 1874, *p.* 301.

114.—THE ANATOMY OF THE PLACENTA.

By Dr. T. Snow Beck, F.R.S.

The account originally given by Dr. William Hunter, and known as the Hunterian doctrine, is perfectly accurate, that the cavernous structure of the placenta is in direct communication with the canals of the sinuses or veins, and that these vessels carry the blood away from the placenta, which is brought there by the utero-placental arteries. Such being the facts, there is no such thing existing as this "feeble wall" or "delicate membrane," either *at* or *forming* the line of separation; nor are there any structures, whatever name may be applied to them, or wherever their exact situation may be said to be, which in any way obstruct or prevent the flow of the blood direct from the placenta into the uterine sinuses or veins. Nor is there any such membrane ever seen *at* or *forming* the inner surface of the uterus after parturition, which has any influence on "possibilities of hemorrhage," or obstructs or prevents the absorption of any fluids which may exist on or at the inner surface.—*Obstetrical Journal, Feb.* 1874, *p.* 743.

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115. A CONTRIBUTION TO THE THEORY AND PRACTICE OF TRANSFUSION.

By Dr. Robert Barnes, Obstetric Physician to St. Thomas's Hospital, Examiner in Obstetrics and Diseases of Women to the Royal College of Surgeons.

[Although by the topical application of perchloride of iron the cases in which transfusion becomes necessary may, we hope, become very rare, yet it is a subject the consideration of which is still important.]

The following case, although not successful, is not without interest in several points:—A short time ago I met Dr. Devereux, of Tewkesbury, at a case of extreme exhaustion

from secondary post-partum hemorrhage. At one time it was thought the patient was dead, so utter was the prostration. The pulse rallied now and then, but often flagged so that it became imperceptible; the respiration was laborious and frequent, so loud that it could be heard in the adjoining room; the face and extremities were cold; the voice almost extinct. She swallowed beef-tea and port from time to time, but soon vomited all. This was about 8.30 p.m. The extreme prostration, and the hopelessness of getting any nutriment absorbed either by stomach or bowel, led us to look upon transfusion as the last hope. Discussing the means of accomplishing this, we found the best we could do was to use an aspirator-syringe. This had to be fetched at a distance of five miles. In the meantime I had got everything ready. The gardener offered an arm. At 10 p.m. the operation was carried out. The syringe being made on the principle of the stomach-pump, with reversible action, answered extremely well. We filed down the point of the aspirator-trocar, and this served very fairly for insertion into the patient's vein. As circumstances left no choice between immediate and mediate transfusion, so were we compelled to defibrinate the blood. The man who yielded the blood was fat, and his veins small, so that it was with difficulty we obtained four ounces. This was defibrinated by whipping with a silver fork and filtering through a cambric handkerchief. Believing that one factor in the danger attending these cases of prostration is the merely dynamic one arising from the absence from the heart and vessels of fluid to act upon, and considering that we had so small a quantity of blood at our disposal, I first filled the syringe with a solution of phosphate of soda, carbonate of soda, and chloride of sodium, at a temperature of 100° F. Pumping this through the syringe also served to get rid of air from the apparatus. The vein in the patient's right arm was found by pinching up a fold of skin, transfixing it transversely to the course of the vein, and then dissecting through a little fatty cellular tissue. It was then pinched up by forceps, and a sufficient opening made into it. The trocar was passed one inch and a half into it. The injection was then slowly made. The blood was partly mixed with the saline solution; altogether about six ounces of fluid, including all the blood, was thrown in. Almost instantly the pulse improved, warmth spread over the body, the face and manner became more natural, the respiration became tranquil, and we were tempted to hope that she might pull through. We prepared everything for repetition of the transfusion in the morning should the condition be favourable. But the rally was short; the respiration again became moaning, rapid, and laboured; the pulse flagged, and the patient sank at 1.30 a.m., about three hours after the injection.

Amongst other reflections suggested by this and similar cases are the following:—

1. The vomiting of the beef-tea and wine, which had been accumulating in the stomach from repeated exhibition in small doses, in an unchanged state, and the obvious failure of any restorative effect, are proofs that the vital power is so low that nothing is to be hoped for in the way of replenishing the empty circulation from absorption.

2. The rapid and laboured respiration is evidence of the craving of the system for oxygenated blood. Air goes in and out of the lungs with great rapidity, but it has nothing to act upon. Its obvious effect is to accelerate the cooling of the body.

3. When we see things at this point—extreme exhaustion, feeble or imperceptible pulse, rapid laboured breathing, non-response of the system to nutriment and stimulants administered by the stomach or rectum, and vomiting and great agitation,—we may fairly conclude that direct injection into the vessels is indicated.

4. Blood may be used either whole or defibrinated. The choice will be governed by circumstances. If we have Aveling's or other proper apparatus to practise transfusion from arm to arm, so that the blood passes direct from giver to receiver without ever being exposed to the air or allowed to rest, this will often be the preferable plan; but where the proper apparatus is wanting, as it is likely to be, defibrination is better.

5. The experience of Dr. Little in cholera cases, and other considerations, show that saline solutions may be injected into the veins with advantage. I believe that they may, with great utility, be used to supplement or to dilute the blood used in transfusion for loss of blood, and that not only is the saline fluid useful by virtue of its chemical properties, but also by its physical quality of bulk, supplying a volume of fluid upon which the heart and vessels can act more effectually.

6. That transfusion will be more and more extensively resorted to appears to me certain. Its extended use, however, must greatly depend upon a general understanding of the conditions that call for the operation, and of the various modes of performing it. So long as it is considered necessary to have a special apparatus, the operation must be performed rarely. But let it be understood that blood either whole or defibrinated will do, and that almost any syringe can be made to answer, and the great obstacle to the extension of the operation will have vanished. Nothing can be more easy than to furnish every stomach-pump case with a flexible tube to which a transfusion-canula is attached. As no practitioner of medicine

ought to be without a stomach-pump, the *necessary* apparatus will never be wanting. I should not hesitate to use an ordinary Higginson's syringe.

7. Here, as in all great emergencies, an essential condition is to *act in time*. Just as the perchloride of iron may be used too late, so may transfusion or injection be delayed until the nervous centres will have lost the power of responding to the fresh pabulum supplied.—*Lancet, Jan.* 3, 1874, *p.* 6.

116.—THE TREATMENT OF VASCULAR GROWTHS OF THE URETHRA WITHOUT OPERATION.

By Dr. ARTHUR W. EDIS, Assistant-Physician to the Hospital for Women, Soho Square.

[The treatment of these vascular growths, which are generally near the urethral orifice, and are productive of great suffering, is exceedingly unsatisfactory.]

The application of the pernitrate of mercury is attended by much pain and inconvenience, and cannot always be resorted to. Nitrate of silver, in my experience, is not sufficiently potent; it produces too much irritation, and does not sufficiently destroy the vascular growth—in many cases seeming to increase rather than diminish it. The actual cautery cannot well be applied unless the patient be at home; and the various other agents, such as potassa fusa, nitric acid, &c., are too painful to be of general application.

To obviate this difficulty, I was induced to try the effects of *chromic acid*, and was surprised to find how little inconvenience it occasioned, and at the same time how efficacious it was in destroying the growth. I have in numerous instances applied a saturated solution, and with the most marked benefit. My usual plan is to roll a little cotton-wool round the end of a small piece of wood (an ordinary match will do); to soak it for a quarter of an inch in the acid solution; and then to carefully yet firmly press it upon the vascular growth until the surface becomes distinctly shrivelled; the surrounding parts being guarded by cotton-wool soaked in solution of carbonate of soda, which is also applied to the growth subsequently, to neutralise any excess of the acid. Slight burning pain is produced, but seldom sufficient to need any employment of morphia, either hypodermically or as a local application.

The sensitiveness of the tumour is almost completely destroyed, some soreness being all that is complained of. In about a week's time, a second application should be made; and, depending upon the size of the growth, two or more subsequent applications, at a week's interval, will generally be sufficient to complete the

cure, which, as far as I have been able to observe hitherto, is permanent. *Carbolic acid* in the solid form, or as a saturated solution, also serves to mummify these growths, and causes far less inconvenience than many of the numerous agents recommended. My experience with this is not yet sufficiently great to enable me to speak more positively, but I believe it will be found to be a very useful addition to our list of remedies in these cases. I append a few cases where the chromic acid had been employed.

Case 1.—J. C., aged 37, married sixteen years, without children, was first seen on October 16th, 1873. She complained of smarting pain on micturition, was obliged to get up several times in the night to pass urine, experienced much pain on walking or exertion, had a slight discharge, occasionally tinged with blood, and suffered much inconvenience *in coitu*. On examination, a vascular growth was discovered bulging from the urethra; it was sessile, being attached to about two-thirds of the circumference of the passage; it was exquisitely sensitive, and bled readily when touched. A saturated solution of chromic acid was carefully, but thoroughly, applied, the pencil being pressed firmly on the surface. Some slight inconvenience was caused at the time, but this passed off after a few hours. A mixture of bicarbonate of potash and tincture of belladonna was prescribed, together with a soothing lotion.

On October 23rd, she reported that the pain on micturition was less; the urethra being still vascular, the chromic acid was reapplied, Bromide of potassium was given in place of the other mixture.

On November 6th, the chromic acid was again resorted to, and caused little or no inconvenience.

On November 20th, the urethra being still somewhat vascular, a strong solution of carbolic acid was applied, after which no further local means were resorted to.

Case 2.—A. H., aged 63, married forty years, without children, on admission, September 18th, 1873, complained of scalding pain on micturition, which had been present two years. She had a constant discharge, and the linen was often stained with blood; she lost a good deal a week since; she thought she had "a gathering in the womb." On examination, a vascular growth was detected at the orifice of the urethra, very sensitive. Chromic acid was applied; very little inconvenience was caused by it, and, when seen on October 3rd, she stated that she had been better ever since her first visit. The growth was much smaller. The acid was again applied.

On October 16th, she had no pain at all on micturition, and felt better in every respect. The urethra was still somewhat

vascular, but as there were no symptoms, nothing further was done; and up to January 1874, she had had no relapse.

Case 3.—E. R., aged 47, married sixteen years, the mother of three children, was admitted July 3rd, 1873, complaining of pains in the womb and lower part of the back, pain in micturition and also difficulty, and leucorrhœa. The uterus was found to be normal, but the urethra was very vascular and sensitive. There was no distinct growth projecting, but a general injected condition for about two thirds of the circumference of the urethra. Chromic acid was applied, and again on the 17th, when she stated that the pain on micturition was very much less than it had been before she came.

On the 31st, she had very slight inconvenience on micturition, and, as the vascularity had greatly diminished, no further application was made.

When seen in September, she had no pain whatever on passing urine. She was suffering from a threatened miscarriage.

On October 2nd, the urethra was found to be perfectly normal. She is now nearly six months advanced in pregnancy.

Case 4.—A. J., aged 63, widow, mother of eight children, on admission on April 24th, 1873, complained of smarting pain on passing urine and difficulty in doing so, bearing down pains, and discharge. She has had "pressure and cutting pain with the water for the last five or six months, disturbing her very much at night." The urethra was found to be very vascular, the mucous membrane much hypertrophied. A zinc point was inserted on two occasions, and left in the urethra for some minutes. This caused some pain at the time, but afforded relief afterwards. However, as the vascularity still continued, on June 12th, the pernitrate of mercury was applied.

On July 12th, the burning and smarting pain in micturition still continuing, chromic acid was applied, and produced much less inconvenience than the pernitrate had done.

On July 24th, she stated that she had experienced little or no pain after the former application, and had been much better since. The urethra was much less vascular, but not quite healthy. The acid was reapplied, since when she has had no further symptoms.

Case 5.—M. S., aged 66, widow, when admitted in October 1872, complained of "pain in her privates," desire to micturate every ten minutes, the act being accompanied by much smarting pain. There was a discharge purulent in character, and other symptoms referable to the uterus. On examination, a large vascular tumour, sessile on the posterior wall of the urethra was discovered; it bled readily when touched, and was very sensitive.

An operation being deemed requisite, no local application was

made. Belladonna with an alkali was prescribed internally, and chloral and morphia at night. No bed being obtainable, the pernitrate of mercury was applied on two occasions, at a week's interval.

On November 15th, chromic acid was resorted to in place of the pernitrate, as the pain from the latter lasted several hours after its employment. It was repeated at the end of a fortnight, again on December 12th, and on January 9th, 1873, and February 11th. After this, the growth disappeared entirely, and required no further application, though the frequency of micturition was still more than natural during the night. She was seen in May 1873, and was very much improved, there being very little irritation now.

Possibly this may be nothing new to some present; but as it seems to be a very effectual mode of dealing with these troublesome little growths, I thought it not unworthy of the attention of the Society, as I am convinced it is not generally known, and it deserves a more extended trial.

The cases I have selected are only a few of those in which I have employed it; in many instances, the patients have attended some few times and then ceased to do so; it is, therefore, fair to conclude that they were relieved from their sufferings, otherwise they would have again presented themselves.—*British Medical Journal, April* 4, 1874, *p.* 449.

117.—THE TEACHINGS OF THE GENERAL INFIRMARY AT LEEDS WITH REGARD TO THE OPERATION OF OVARIOTOMY.

By C. G. WHEELHOUSE, Esq., Senior Surgeon to the Infirmary.

Bearing in mind that many of these tumours are of a mixed character, partly solid and partly fluid, we may yet, as a rule, classify them generally under these two heads. Accordingly, in examining an ovarian case, I always direct my attention, in the first instance, to this point; for I know that, according as I have the one or the other kind of tumour to deal with, not only will widely different lines of treatment be required, but that as different an opinion also, as to the advisability of any operation being performed, will probably hang thereon.

Take, for instance, the case of a purely solid tumour first, and consider how great are the difficulties which, on the very threshold of our inquiry concerning it, may await our diagnosis. Consider how many things there are which *may* be, and every one of which *have* from time to time been, confounded with ovarian tumour; pregnancy, extra-uterine fœtation, uterine fibroma, or the many forms of internal disease or displace-

ments to which the uterus is liable; peri-uterine hæmatocele, fæcal accumulation, mesenteric tumour, enlargement and disease of the kidney, enlargement and disease of the spleen—and you will not wonder when I tell you that I have learned, whenever a purely solid tumour is presented to me for examination, that I must never set the case down as one of ovarian disease, until, by an exhaustive process of elimination, I have assured myself that it can be nothing else.

The degree of fixity or mobility of such a tumour is a point to which I have learned to attach great importance, and also the presence or absence around it of ascitic fluid. I long ago learned to believe, and, up to the present time, I see no reason to doubt the correctness of the conclusion that, with a view to ovariotomy, no more favourable arrangement of circumstances can present itself than a solid ovarian tumour, freely movable, and surrounded by ascitic fluid; whilst on the other hand, a solid ovarian tumour, fixed and immovable, I invariably approach with the most serious consciousness of impending difficulty. It is easy, I think, to understand why this should be so. If a solid ovarian tumour be tied in position by adhesions, the probability is, that such adhesions will be pelvic and posterior; they will be situated at the very points at which it will be most difficult to separate them, and, even supposing this successfully accomplished, the raw surfaces produced will occupy the very positions which are fraught with the greatest after-dangers to the patient. It will be into the pelvis, and to the back part of the body, that blood and effused fluids will gravitate; and from this part should they undergo decomposition, they will find the readiest and most direct entrance into the circulation, to the almost certain death of the patient.

I have said that a solid tumour, movable and surrounded by ascitic fluid, has, in my hands, proved the most favourable of all forms for operation; and that leads me to an observation which, I believe, all practical ovariotomists will endorse, though few attempts have been made directly to explain the reason why, after such operations, the patient generally progresses towards recovery in so remarkably quiet a manner. The question whether it is better to perform the operation whilst the patient is in robust health, or to wait until she is somewhat enfeebled by the disease, has been ably discussed both in this country and America; and, whilst all the earlier operators recommended that it should be done under the former condition, nearly all are now agreed that the greatest success has attended such as have been undertaken under the latter. Many reasons have been given for this, but I have nowhere seen it distinctly stated that the difference in the

result is mainly dependent, as I believe it to be, upon an altered condition of the peritoneum.

Pealee comes nearest to this point when he says, "At all events a long distended state of the peritoneum very much diminishes its sensibility and the danger of traumatic peritonitis" (Ovarian Tumours); but neither he, nor any of the authorities whom I have consulted, lay any stress upon the altered structural conditions of the membrane itself. If you consider for a few moments what is the normal condition of the peritoneum, both in structure and as to function, you will have no difficulty, I think, in understanding the ground upon which I assert that I would rather operate in a case where I know that it has undergone structural change than in one in which it is presumably perfectly healthy.

Physiologically the largest lymph-sac in the body, it is, as you are aware, perpetually engaged in the process of absorption; and if, in its unaltered condition, septic fluids be presented to it, their immediate absorption is, of course, the result; and structurally the largest serous membrane in the body, it is, of all others, the most liable to become inflamed under conditions of provocation. Thus, in the performance of ovariotomy with a healthy peritoneum, we have the greatest liability to the two conditions which, of all others, are fraught with the greatest danger to the patient, viz., peritonitis, which alone destroys one-fourth of the cases which die, and septicæmia, which carries off at least as many. If, however, the membrane have undergone such changes, as the result of disease, as either weaken its power of absorption, or so change its structure that it is, in reality, no longer an active serous membrane, by so much, according to my experience, is the chance of the success of an operation improved; and this is a fact which, I think, my colleagues will bear me out in saying was very early recognised in this institution, and has oftentimes served usefully to assist us in forming our prognosis as to probable results in individual cases. I always hail the presence of ascitic fluid in the peritoneum as of highly favourable augury; and even adhesions of lymph, so only that they be soft and easily broken down, generally impress me favourably rather than otherwise. I can in no way, other than on this supposition, account for the perfectly calm recovery of many cases I have seen—which, to a stranger, would have appeared absolutely hopeless—where adhesion after adhesion has had to be broken down or cut through, and in which, notwithstanding, the patient has progressed afterwards with most singular felicity.

In the case of fluid or cystic tumours, considerations of another order regulate my judgment, and, in a measure, my method of procedure also. Adopting the commonly accepted classification

of these cysts into uni- and multi-locular, I have learned to deal very differently with them, as they belong to one or other of these orders. In the case of an uni-locular cyst, I have long held with Mr. Southam of Manchester, that it is advisable, in all cases, to give the patient the benefit of tapping once, at all events, and that for more reasons than one. 1. It occasionally happens in some rare cases, that such a cyst, once fully emptied, may never refill, and the patient may, by this simple proceeding, be permanently cured. Two such cases have happened in my own practice, and I think my friend and colleague Mr. S. Hey will be able to support me in this doctrine by the result of a third case which I saw in consultation with him. A young lady tapped two years ago, has, up to this time, shown no liability to return of the disease, and has since married. 2. When the cyst *does* refill, it often does so very slowly, and, by a repetition of the tapping at long intervals, the patient may not only be kept in tolerable comfort, but the necessity for the major operation, with all its attendant risks, may be altogether staved off. Mr. Southam mentions several such cases, and one in particular, in which the patient only required the operation twice in nineteen years (Ovarian Tumours, 1869). 3. It should be done as an aid to diagnosis. It not unfrequently happens that a case may be so involved in doubt that an inspection of the contained fluid may prove of material assistance to us in this direction.

As a rule, it is not difficult to distinguish between the fluids of ascites and of ovarian dropsy, and thus a single tapping will *often* serve to clear up any doubt that may exist on this point. I say *often*, because it will not always do this. Cases have come under my own observation, and others are recorded, in which it has been exceedingly difficult, even with the fluid extracted, to say which of the two it is.

To tap a manifestly multilocular cyst is, in my judgment, to run a very serious risk, and one which is not generally counterbalanced by the amount of relief likely to be obtained by the operation; and hence it becomes a matter of considerable moment to be able to form a probable diagnosis as between the two forms. To a practised eye and a practised hand, this is, I believe, generally possible, but not in all cases. Hence it sometimes happens that the simple operation of tapping leads to very serious consequences, and occasionally even to death; and the next point to which I wish to advert is the lesson I have recently learned on this point.

If, after tapping a given cyst, it should turn out that we have not been able to empty it completely, what are the dangers which may arise from the procedure? The cyst may inflame; it may suppurate; or it may gangrene; and, under any of

these circumstances, the life of the patient may be brought into imminent danger. In the very last case in which I operated in this institution, my patient positively refused to listen to any proposal to tap her, because, as she said, her sister, who had been, like herself, the subject of ovarian disease, had been tapped and had died. She was quite willing to submit to the major operation, and take all the risks attendant on it, but the minor one she steadily declined.

Now, bearing these risks in mind, I think it should be an invariable rule to take the patient fully into our confidence, to explain to her all the danger that may arise even from tapping only, and to see that she is prepared, should any of the accidents I have named unfortunately supervene, at once to submit to any further procedure that may be deemed advisable for her safety. That we have it in our power, even in that extremity, to make a manful fight in her defence, has been very recently proved by my friend Mr. Teale, who will be able, I believe, this evening to bring forward a case which, I think, he may proudly detail to you as one of the most brilliant triumphs of modern surgery.

A patient, the subject of ovarian disease, was tapped; the half emptied cyst failed fully to collapse; rapid decomposition of the remaining contents ensued, and the case was clearly about to end fatally in a very few hours. At a consultation, it was determined, notwithstanding the apparently desperate condition of the woman that her only chance lay in the immediate performance of the major operation, and that was forthwith done. By Mr. Teale's permission, I am enabled to show you the cyst. In the posterior part of it is a large patch of gangrene, which, in a very short time, must have separated or have given way, and must have caused immediate death; and I am sure that you will join with me in congratulating my friend that, notwithstanding this desperate condition of affairs, he has succeeded in carrying this patient through her difficulties, and that she is now quite recovered.

With regard to the operation itself, I desire explicitly to record my feeling in favour of a full and free primary incision. For a long time, the comparative merits of the large and the small incision were vigorously debated, and each could claim many supporters. On the one hand, it was argued that, if, after tapping and emptying a cyst, it could be withdrawn through a small opening, it was so much gain to the patient; but, on the other hand, it was soon ascertained by those who performed the operation most frequently that, in reality, the risk of peritonitis was quite as great after the small as after the large opening; and, moreover, it was so frequently found necessary to enlarge an opening, originally small, to meet some

unexpected complication, that, slowly but steadily, the larger opening has been gaining ground, until I think, it may be said to be very generally adopted. At all events, I would speak for myself, and I have no hesitation in saying, that I undoubtedly prefer a large opening.

I invariably feel, in commencing an ovarian operation, however well I may have thought the case out beforehand, that I may have to deal with difficulties and complications which could not be foreseen; and, in dealing with such, I greatly prefer to be able to see clearly all I have to do, and I feel certain the advantage is great to the patient, as well as to the surgeon. When, for instance, he has some unlooked-for adhesion to separate, or some bleeding vessel to secure, or some unexpected difficulty to encounter, how much more safely he can do it all when he can see what he is about, than if he be groping about in the dark; and I do not believe that, when once the peritoneum is opened, it makes the slightest ultimate difference whether the opening be three inches long, or six.

Moreover, I think, much less harm is done to the peritoneum itself whenever a tumour is lifted without strain through a free opening, than when one is liberated only after force has had to be used to drag it through a small incision. I do not think this position could be better illustrated than it was in another case in which I recently assisted Mr. Teale, and where through the whole operation, up to a certain point, all went as smoothly as we could any of us desire. A monocyst, easily emptied, turned out almost of its own accord, and seemed just on the point of escaping altogether from the body, when at the very furthest point behind, it was held back by a firm adhesion. To liberate it, this had to be divided; in doing so, a large feeding vessel was severed, and, unless we had had a tolerably free opening, I think Mr. Teale will agree with me in thinking that, to secure that vessel would have been almost, if not quite impossible. I do not consider it necessary, in all cases, to make an incision from ensiform cartilage to pubes; but, judging by the size of the tumour or cyst I have to remove, I always so calculate its length as to give myself free space for all reasonable manipulation.

As to the best method of dealing with adhesions; if they be recent and soft, they are easily broken down; and, following them carefully with the finger and the eye, to detect any vessel that may seem inclined to bleed, I generally so deal with such; but, if the tumour be firmly adherent to the abdominal walls, or to any of the viscera, this cannot be done. If they be old and well organised, and pretty certain to live, I much prefer to cut out the portions of the cyst, and leave them attached than to attempt to dissect them away. In doing this, it is impossible

to avoid a great deal of diffused bleeding at the time, and the oozing is very liable to continue after the completion of the operation. There is also the further risk, in attempting to loosen such adhesions, of injuring the organ to which they are attached; it is easy thus to tear into an intestine. I have seen the bladder lacerated, and I have heard of other accidents which have all proved fatal. On the other hand, I have adopted the method of leaving portions of the cyst attached, and I have never, in any instance, seen any evil arise from doing so. Upon the best method of dealing with the pedicle, more than anything else, hangs the success or failure of most cases. We have dealt with the pedicle in this institution, I believe, in every known way. We have used the clamp in all its forms; the ligature, both cut short, and left hanging out of the wound. We have retained the stump in the lower part of the incision, and we have let it go free. We have tried the ligation of the whole pedicle, and of individual vessels, the searing iron, and torsion. Every method has succeeded in some and failed in other cases; and so, feeling thankful that I have so many strings to my bow, I try to deal with each case according to its special requirements. I am careful, above all things, if it be possible, to avoid having any drag on the stump; and when, having severed a tumour, and arrested all hemorrhage in the stump, I feel that I can safely drop it into the pelvis, and see it no more, I am the most easy about it.

The two methods from which I have decidedly seen the happiest results have been "searing" and "torsion." The former is effected by seizing the pedicle near the uterus with a clamp, and then cutting off the tumour with the searing iron, and carefully burning away every atom of the eschar; the latter by simple twisting. The uterine portion of the pedicle, having been grasped by a clamp made specially for the purpose, is firmly held, and the end from which the tumour has been detached is enclosed in a second clamp, and the portion is then slowly and deliberately twisted off.

In using either of these methods, I am still careful, when the clamps have been removed, to search carefully over the end of the stump, and if I see any bleeding point to tie it; and having done this, and assured myself that all is safe, I delight to feel that, henceforward, I have done with the pedicle.—*British Medical Journal, March* 21, 1874, *p.* 373.

118.—A PRACTICAL POINT IN THE OPERATION OF OVARIOTOMY.

By Dr. ATLEE, Philadelphia.

Dr. Atlee calls attention to the following very important practical point in the operation of ovariotomy. It is this:

immediately after making the incision through the walls of the abdomen, the index finger should be passed up to the region of the umbilicus, and if it can be swept freely across from side to side it must be within the abdomen. This, of course, is an easy matter when no adhesions exist. It is always possible in parietal adhesions, when the finger is inside of the peritoneum. It is not possible, without the most unwarrantable violence, when the finger is between the layers of the abdominal parietes. The non-observance of this rule has led to the separation of large portions of the peritoneal layer of the walls of the abdomen, even when no adhesions existed, the operator having mistaken the peritoneum itself for an adherent cyst-wall. When, however, parietal adhesions do exist, the mistake may be more excusable and more readily made, particularly in such a case as the one just related, where the peritoneum is thickened and more strongly incorporated with the cyst-wall than with the wall of the abdomen. The most convenient and infallible test of being within the abdomen is *the ability to freely move the finger to and fro past the umbilicus.—Philadelphia Medical Times.—Edinburgh Medical Journal, Nov.* 1873, *p.* 460.

119.—CATELECTROTONUS OF THE OVARIES IN THE TREATMENT OF AMENORRHŒA.

By Dr. JULIUS ALTHAUS, Physician to the Hospital for Diseases of the Nervous System.

Amenorrhœa is a condition which may—according to the causes by which it is produced, and the circumstances in which the patient is placed—have a very slight or a very considerable importance. We know that it is in many cases simply a symptom of insufficient sanguification, as when it occurs in chlorosis, phthisis, or after severe hemorrhages or long-continued discharges, which have exhausted the system. Under such circumstances, common sense, and also the highest gynæcological authorities, teach us that any local stimulation of the sexual organs, in order to produce a menstrual flow, would be improper; and that the general condition, of which amenorrhœa is merely an expression, must be rectified in the first instance. Very frequently the ovarian function is re-established simultaneously with the restoration of the general health. In other instances, however, the catamenia are suddenly stopped in consequence of a chill, fright, anxiety, and depressing emotions; and the function may then remain in abeyance long after the cause by which it had been arrested has ceased. We can then no longer be sanguine that we shall be able to restore the menstrual flow by acting on the system generally; and local stimulation of the sexual organs is generally found requisite.

If amenorrhœa occurs in spinsters who are no longer likely to marry, or in married women who have already borne a number of children, the condition is not one of very great importance; but where we meet with it in married and childless women the aspect of the case is considerably altered. As it may be assumed that, when the catamenia are arrested, ovulation has likewise ceased, the woman with amenorrhœa is, as it were, prematurely unsexed; and the re-establishment of the menstrual flow then becomes a vital object for the would-be mother. In cases of this kind, where the general health is as a rule excellent, the usual medicinal and surgical treatment of amenorrhœa often fails to rouse the dormant function of the sexual organs; and the various forms of electricity then step in as the right thing in the right place. The most effective mode of applying electricity I believe to be the induction of catelectrotonus of the ovaries; and the following may be considered a typical case:—

An American lady, aged 37, had her first confinement in March, 1871. The child was full-grown, but still born. The fœtal movements had suddenly ceased about a week previous to delivery, without any accident having occurred to the mother. The body of the child was found to be decomposed when born and forceps had to be used. The mother was very ill for a considerable time afterwards, and she never saw any further trace of the menstrual discharge, although previous to her pregnancy she had always been perfectly regular. She underwent much treatment, and ultimately consulted Dr. Fordyce Barker, of New York, who pronounced the case to be one of "atrophy of the ovaries," and said that nothing could be done to restore the function of those organs. In May last the patient came to England, and being very anxious to have children, she consulted Mr. Spencer Wells, who discovered a slight degree of retroflexion of the uterus, which he replaced by the sound, and kept up by a pessary. He believed the amenorrhœa to be owing to a torpid condition of the ovaries, and sent the patient to me with the request to use electricity for rousing their function.

The general health of the patient was at that time quite satisfactory, and therefore no medicine was given. There had never been any vicarious menstruation or molimina. I induced catelectrotonus of the ovaries by placing the negative electrode of the constant battery alternately to the right and left ovarian region, putting the anode alternately to the lumbar spine and to the os uteri by means of an insulated sound. A current of from fifty to sixty cells of Daniell's battery was used, and the action of it kept up for fifteen minutes at a time. After a few such applications, the patient said that she felt the same sensations which she had always felt just before the period used

to come on—viz., persistent uterine pain and frontal headache, chiefly in the right side of the head. A slight mucous discharge from the womb began at the same time. The galvanic current was now used on twenty-two different occasions, and the general sensations of the patient at the end of that period were such as to fully convince her that the catamenia were impending.

This was towards the end of June last. External reasons now obliged her to leave London, and I did not see her again until November 7, when she informed me that she had been disappointed in her expectation. The sensations which she thought premonitory of the occurrence of menstruation had disappeared within the first few days after discontinuing the galvanic treatment; the discharge from the womb had likewise ceased, and no sign of the period manifested itself. She was anxious to give the treatment a further trial, and it was therefore now resumed. Within the first few days the symptoms, which she had felt in summer, returned—viz., uterine pain and frontal headache. Shortly afterwards a thick mucous discharge from the womb was established, which was on one occasion sanguinolent, and so acrid that it made the inside of the thighs sore; for the relief of which cold cream was not sufficient, but free applications of the benzoated ointment of oxide of zinc were found requisite. The current was now used thirty times. The sensations previously described ceased, while the discharge persisted.

The treatment was now interrupted for ten days, and resumed on December 22. After three more applications, the period came on Christmas-day, to the intense gratification of the patient. The flow was, for the first twelve hours, quite as abundant as it had ever been before. It then diminished, and ceased altogether four hours afterwards; but was succeeded by a dark mucous discharge which lasted two days longer. As it appeared to me that this first response of nature had been very imperfect, I advised another short course of treatment just previous to the time when the next period would be expected. The patient accordingly came to me again on January 13, and had eight more applications of the current. Menstruation returned on January 22; it was of a particularly good character, and went on abundantly for four days. As, therefore, not only a satisfactory quality, but also decided periodicity of the ovarian function appeared now to have become established, I discharged the patient from further attendance, and she left England for Italy shortly afterwards.

The result of the treatment in this case was all the more satisfactory as the amenorrhœa had already lasted nearly three years, the patient was no longer very young, and there seemed to be no prospect of other means proving effectual. Indeed, so

able and experienced an observer as Dr. Fordyce Barker had pronounced the case to be incurable. Its ultimately successful issue, however, shows that the induction of catelectrotonus of the ovaries is a powerful remedy, by means of which, if perseveringly employed, the function of the ovaries may be restored even after having been in abeyance for years.—*Medical Times and Gazette, March* 14, 1874, *p.* 287.

120.—ON UTERINE THERAPEUTICS.

By Dr. LOMBE ATTHILL, Fellow and Examiner in Midwifery, King and Queen's College of Physicians, Obstetric Physician to the Adelaide Hospital, Dublin.

[In the following paper the writer speaks only of that important class of remedies which are applied directly to the vagina and uterus.]

Syringing the vagina with water or with medicated fluids is an old and popular remedy for nearly every form of uterine disease, a remedy which, though often of great value if properly performed, and practised in suitable cases, is as often utterly useless, and occasionally positively injurious. Thus, an elastic enema-bag, capable of holding from six to ten ounces, is commonly employed for the purpose; such an instrument is quite useless. But occasionally a worse, because a positively dangerous instrument, is employed, namely, a glass syringe, the end of which is perforated with five or six holes. Not long since I was requested to see a woman to whom such a syringe had been supplied. The glass being thin, the instrument broke in the vagina, and several pieces of broken glass remained in that canal, causing intense pain to the patient. By slowly and carefully introducing a Ferguson's speculum I was enabled to extract one by one through it the fragments of the syringe, and no serious consequences followed.

The syringe to be employed for the purpose of vaginal injections should be one capable of throwing up a continuous stream. Such syringes are commonly known as "the syphon syringe," or "Higginson's syringe."

When using the syringe the patient should, if possible, lie on her back, the hip resting on a bed-pan, which receives the fluid as it escapes from the vagina, but the majority of patients object to this plan, which necessitates the presence of an assistant, and then you are obliged to permit her to inject the fluid from a vessel placed in front of her, or in a foot-pan, or bath over which she sits. This is a very much less efficient method, for the fluid escapes from the vagina too rapidly, and does not distend that canal, as it is desirable it should. In

cases where there is not any urgent reason for the use of medicated lotions it is often a good plan to direct the patient to use her syringe while sitting in a warm hip-bath. I have found this method most efficacious in allaying vaginal irritation.

Having thus pointed out to you the method of syringing the vagina, it is further important for you to consider of what temperature the fluid to be injected should be, the quantity to be used at each time, the medical agents to be so employed, and their strength.

As a rule, I recommend you not to inject any fluid into the vagina which is perfectly cold; doubtless a perfectly cold fluid is more tonic, if I may use that expression, than a warm one could be, but the object of injections is, as a rule, to allay irritation, and not to give tone to the vaginal walls; that will soon follow as a result if you remove the local affection. Besides, I have seen very unpleasant and even serious consequences follow the injection of cold water into the vagina. Thus, severe uterine colic, and intense pain above the pubes occurred as an immediate result in one case, and in another so grave were the symptoms that life was endangered from an attack of pelvic cellulitis which followed the injection into the vagina of cold water, ordered with the view of checking profuse menstruation. I recommend you, then, to direct that the fluid to be employed be used at about blood heat, and when vaginitis is present, at even a somewhat higher temperature. The quantity to be employed should be considerable. If tepid water be used no better plan can be adopted than to make the patient use the syringe while sitting in a warm bath. If this cannot for any reason be done, she may sit over a foot-pan. Having a large supply of the water to be injected in a vessel placed in front of her, as already mentioned the recumbent posture is the best, for if it be adopted we may feel sure that the fluid will reach the highest part of the vagina, which I am satisfied is seldom the case when a sitting posture is adopted; but this method is too troublesome to be carried out easily in private practice, and is objected to by most females. Nor am I sure that the results are commensurate with the trouble.

The medical agents employed for vaginal injections are very numerous. I, however, restrict myself to a few. I have found the solutions of alum and of the sulphate of zinc so frequently to aggravate the patient's sufferings when vaginitis was present, that I now in such cases seldom or ever employ either. They coagulate the albumen which enters so largely into the composition of leucorrhœal discharges. If you examine a patient any time within twenty-four hours after she has used an alum injection you will find in the vagina a number of hard masses formed by the coagulation of the discharge, and these often

cause much discomfort. Borax is a better agent, but it sometimes causes irritation, though in a less degree. A drachm of borax to the pint of water is the strength I usually direct to be used.

Where the object is to soothe and to allay irritation this is best effected by the use of an infusion of tobacco. Tobacco must, however, be used with caution. Some patients are peculiarly susceptible of its action, especially those in whom the orifice of the vagina being narrow some of the fluid is retained in the canal. Begin, therefore, by infusing fifteen grains of the unmanufactured leaf in a pint of boiling water. If this produce no unpleasant effect increase the strength to thirty—even sixty, grains to the pint. In many cases the addition of a drachm of borax to each pint of the infusion greatly increases the efficacy of the treatment. Many patients, however, are unable to use the tobacco at all, as even a very weak infusion causes nausea and faintness. I therefore recommend, when this is the case, or where you fear to run the risk of causing any discomfort, to substitute for tobacco an infusion of hops, directing an ounce of hops to be infused in a pint of boiling water with or without the addition of borax, as you may deem advisable.

Cases are, however, frequently met with where no vaginal inflammation or even irritation exists, but where profuse and weakening leucorrhœal discharge is constantly being poured out, which it is necessary to check; here astringents, such as alum or zinc, in the proportion of sixty grains to the pint of tepid water, prove often most useful. Should they irritate you will frequently find the decoction of oak bark serviceable. Warn your patient, however, that the infusion of oak bark stains linen, for ladies will not be pleased to find their underclothing or towels covered with ugly stains. This reminds me to give you a caution respecting the use of the solution of nitrate of silver. A few years ago this was almost the only remedy employed in the treatment of uterine disease. I can with confidence say that as an application in cases of disease of the body of the uterus or of the cervix, it is perfectly useless. In cases of vaginitis it may be employed with advantage. It must be applied through a speculum, the surface of the vagina being brushed over with a solution containing twenty or thirty grains of the salt to an ounce of water. The application may be repeated at intervals of two or three days. I now seldom employ the solution of nitrate of silver, as I look on its use in the majority of cases as a mistake, and, moreover, I believe I can obtain better results by other means.

Of all the agents which are applied to the vagina for the relief of inflammation or congestion of the canal, glycerine,

without doubt, is the most valuable. A small roll of cotton-wool will absorb five or six drachms of glycerine; you fasten to this a strong thread or piece of twine, introduce it through a speculum, and leave it in the vagina for twelve or even twenty-four hours, directing your patient to withdraw it at the expiration of that time by means of the string which is left hanging outside the vulva. Glycerine thus applied produces a copious watery discharge, which has a marked effect on the mucous surfaces in immediate contact with it. Thus, after its application the vagina and vaginal aspect of the cervix uteri appear pale, and the copious discharge seldom fails to relieve, for the time at least, that distressing sense of heat which is complained of in severe cases of vaginitis. In less acute cases the addition of ten grains of tannic acid to the ounce of glycerine often proves useful, but if used before the acute symptoms subside, it may cause increased irritation. Be sure whenever you use glycerine to warn your patient that she is to expect a copious discharge, otherwise the great flow which often comes on almost immediately will cause much alarm.

Such applications are, however, quite insufficient when an extensive granular condition of the lips of the os uteri exists; such a condition is always coexistent with a similar state of the canal of the cervix, and frequently of the interior of the uterus also. In these cases it is necessary to carry your treatment into the cavity of the body of the uterus, even up to the very fundus, for it is seldom possible to cure disease of the interior of the uterus unless applications be made directly to the diseased surface. This is the department of uterine surgery in which the greatest advance has been made of recent years. Cases are daily being treated successfully now which were looked upon as incurable but a short time ago. Much has been done in the treatment of these obscure affections, but much still remains to be done; however, I am very hopeful of our future, and believe that the incurable ones will ere long form but a small percentage of the cases of uterine disease.

Numerous medical agents are now employed in the treatment of disease of the body of the uterus. These may be used in the form of fluids, of solids, or of ointments. I mention them in what I consider to be the order of their value.

With respect to fluids I give you one caution: do not inject them into the uterus; such a method is fraught with great danger, and except that it is generally easy of execution, possesses no advantage.

The fluids most commonly employed in the treatment of intra-uterine diseases are a saturated solution of carbolic acid, the tincture of the perchloride of iron, tincture of iodine, the pernitrate of mercury, chromic acid, and the fuming nitric acid.

A solution of nitrate of silver is also sometimes used, but I believe it to be altogether inefficacious.

Carbolic acid is a mild, but by no means a painless application; applied to the vaginal surface of the cervix it produces a very superficial slough, its effects passing off in twenty-four hours. Applied to the interior of the uterus its effects are equally superficial and transitory. It is therefore useful in cases where you desire to apply a mild stimulating caustic, but it is not suitable when it is necessary to destroy the so-called granulations which in severe cases cover the vaginal surface of the cervix and extend into its canal; nor where an unhealthy condition of the mucous membrane lining the body of the uterus, the result of chronic endometritis, exists, a condition which often gives rise to profuse menorrhagia. It is best applied by means of a flexible silver or copper probe round the end of which is wrapped a layer of cotton; this can be passed into the uterus to the desired depth. When carried beyond the os internum the carbolic acid sometimes causes pain, which, however, soon subsides.

The perchloride of iron is an admirable styptic, and, as such, should be used when it is desirable to check uterine hemorrhage. You can apply it in the same manner as the carbolic acid; but it is generally better to saturate a small roll of cotton with the tincture (or, as being less irritating, with a saturated solution of the perchloride in glycerine). Pass this up through a speculum, and place it in contact with the os uteri, and then, outside this, another and larger pledget of cotton well soaked with glycerine. Both these should be removed within twelve hours of their application. I have seen a very deep slough produced in the vagina in a case where the cotton, saturated with the perchloride, was accidentally left in the vagina for two days. When it is desirable to check hemorrhage depending on a granular condition of the cervix, the perchloride of iron is a very valuable agent. Iodine has been used for the same purpose; it will sometimes answer, but it is less certain in its effects. The pernitrate of mercury is a powerful and active caustic. It has been recommended by some practitioners as an application in cases of malignant disease. I never employ it, because I believe I have at my command a caustic equally, if not more efficacious, and one, at the same time, much safer, for very severe salivation has followed the use of the pernitrate in persons susceptible of the peculiar action of mercury.

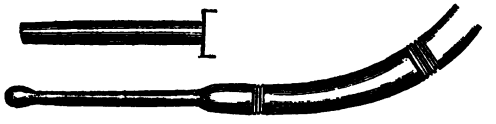
Chromic and nitric acid are nearly identical in their action. The former is, however, in my opinion, more uncertain in its effects; it is also more irritating. I therefore prefer the nitric acid. Its application causes very little, indeed, in general, no pain; it produces but a superficial slough, and has often a wonderful effect in bringing about a healthy condition of the

mucous membrane lining the body and cervix uteri. It also, in many instances, exerts a directly sedative influence, allaying the severe pain and vesical irritation so constantly present in cases of endometritis.

No matter which of these fluid caustics you may select, certain rules applicable to all should be borne in mind. In the first instance, local inflammation, indicated by tenderness of the uterus when touched, should, if present, be removed, or at least mitigated, by appropriate treatment before any of them be used. To effect this the cervix, if soft and engorged, should be punctured, or if enlarged and indurated, leeched.

When it is desirable, as is frequently the case, to carry the application up to the fundus, this should, if possible, be done through a canula or tube, with the double object of preventing the agent selected from being weakened by admixture with the secretions during its passage through the cervical canal and by contact with its walls, and also of protecting the healthy structures from the action of the caustic which may be employed, for it must be borne in mind that the mucous membrane lining the cavity of the uterus may be, and often is, diseased, that lining the cervical canal being in a perfectly healthy condition. It is therefore all-important that the healthy structures should be protected from the action of the caustic.

With the view of effecting this object, I have devised an instrument of very simple construction. It consists of a short tube or canula, made of platinum, and of a curved stilette, which fits the canula accurately, and which is fixed to a box-wood handle. The instrument being introduced into the uterus in the same manner as the uterine sound, the stilette is to be



withdrawn, and the canula being held steady, by means of a pair of long forceps a copper, or better still, a platinum rod, dipped in the agent selected, is to be passed through the canula up to the fundus. You have all repeatedly seen me treat cases of endometritis in this manner, and with the most satisfactory results. I may refer you to the case of a patient at present in the house as an example. She is a widow, aged 45, was admitted on the 15th of last month, suffering from profuse menorrhagia. A careful examination of her case convinced me that the hemorrhage depended on a granular condition of the mucous membrane lining the cavity of the uterus. The vagina

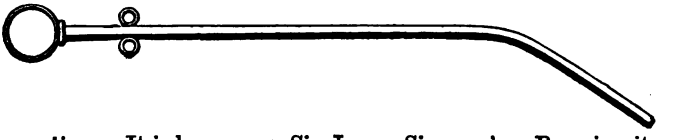
was plugged to arrest the flow, which had continued incessantly for fifteen days, the interval between the commencement of this and the termination of the preceding period having been but ten days. On the subsidence of the flow the canula was introduced with the greatest ease. This was done in the manner I usually adopt, the os being exposed by means of a full-sized Ferguson's speculum; the stilette, armed with the canula, was with ease passed through the cervical canal into the uterus. There is, in general, little difficulty in effecting this, for in suitable cases the cervical canal is patulous, or at least, relaxed. In the case under consideration this was so. Having introduced the instrument and withdrawn the stilette, I passed through it this platinum rod, round the end of which was firmly wrapped a roll of cotton saturated in the fuming nitric acid; no pain followed this application, which was repeated after the lapse of a week. The result was most satisfactory. The menstrual flow did not reappear for five weeks, and then lasted for but five days, the quantity lost also being moderate. Bear in mind that for months past this woman had never been free from profuse and debilitating hemorrhage except for a few days at a time. But do not suppose that because I have succeeded, by two intra-uterine applications of the fuming nitric acid, in stopping the excessive loss, that this woman is cured; her condition is improved, but she is not cured. A relapse will certainly occur if the same application be not repeated, and that too, several times.

Now one word as to the details of this operation—if that be not too dignified a name for the proceeding, for you will fail in your attempt to carry out this method of cauterising the interior of the uterus successfully unless you attend to various little points. The first is that you take care to grasp firmly the little projecting ear of the canula with a pair of long forceps before you withdraw the stilette. If you do not do so, one of two things will happen: either the canula will slip out of the cervix, or, if the os be patulous, as is frequently the case, it will disappear *in toto* within the cervical canal. Doubtless it will soon reappear; but it is not then always easy to grasp it, and it will sometimes slip behind the speculum, or, if grasped, may be found full of mucus. By holding the canula firmly with the forceps these troubles will be avoided. Next, and even more important, is the fixing of the cotton firmly on the end of the probe. Draw out the cotton, moisten the tip of the rod, catch but a few fibres of the cotton at first, and roll the rest slowly and evenly on. This is better effected by rotating the rod than by rolling the cotton round and round it. If these directions be not attended to the cotton will wrinkle up as it passes through the canula, and will render the passage of the rod

impossible, or, if loosely put in, may be left behind in the uterus when the rod is withdrawn. Neither of these accidents will ever occur if the directions I have given be followed.

These directions apply equally to all liquid caustics used for the purpose of intra-uterine medication, and the success of your treatment will depend very much on the dexterity with which you carry it out. If there be too much cotton rolled round the probe, or if it be loosely rolled on, the rod will stick in the canula, and you will have to withdraw it and re-introduce it again, or if you take up too much of the caustic on the cotton it will trickle down, and may cause a troublesome sore to form on the vagina; so that to carry out this method, simple though it be, skill is needed and must be acquired.

Of the solid caustics, the nitrate of silver and sulphate of zinc are the only ones I use. You can insert these through the canula I have described; but it is better effected through this *porte*



caustique. It is known as Sir James Simpson's. By using it you can dispense with the speculum. Ten grains of the nitrate of silver or of the sulphate of zinc, the latter in the form of "zinc points," as suggested by Dr. Braxton Hicks, are to be introduced through it up to the fundus, and left there to dissolve. Either of these caustics so used is liable to cause pain, seldom however severe in character; this too can be in some degree at least averted by placing a pledget of cotton saturated with glycerine in the vagina. I use both these agents occasionally, but less frequently than formerly, for since I have devised the means of applying the nitric acid without previous dilatation to the interior of the uterus, as is effected by means of the canula, the results have been so satisfactory that I now seldom resort to the use of the solid caustics. Of the use of ointments I have no personal experience; they are more difficult to apply than either the fluid or solid caustics named. Dr. Barnes, however, considers them to be often of great value in some cases; doubtless they sometimes are so.

It is occasionally advisable to destroy the tissues of the cervix to a greater depth than can be effected by means of the nitric acid. For this purpose two agents are employed—namely, caustic potash, or potassæ c. calce, and the actual cautery; the former is eminently useful in those cases where the lips of the os uteri are in a state of granular erosion. You have seen me use it with the very best results. As I have in a previous lecture

explained to you the mode of applying it, I shall not dwell on it now further than to remind you that it must be used cautiously, and that the vagina must be protected from the action of the caustic by the insertion of a pledget of lint saturated in vinegar under the lower edge of the cervix. The actual cautery is not much employed in this country, but in America its use is warmly advocated. Dr. Gaillard Thomas states that, according to his experience, "of all the means of counter-irritation for removing chronic parenchymatous congestion, and causing a diminution in the size of the uterus by stimulating absorption, this is the most efficient and least objectionable as to its consequences." He uses a small steel rod terminating in a disk not much larger than a split pea. This heated in a spirit lamp he applies for ten or twenty seconds to the cervix, so as to create a small slough, re-heating and re-applying the cautery so as to cauterise the cervix in two or three places, one at either side of the os uteri.

Dr. Getchell, of Philadelphia, also advocates the use of the actual cautery in cases in which the cervix uteri is hypertrophied and indurated, but instead of a steel rod he employs charcoal sticks made of nitrate of potash twenty grains, charcoal seven drachms, powdered acacia one drachm, and water sufficient to make into a paste. This paste is to be formed into sticks of any required diameter and length. Dr. Getchell uses them of about the diameter of the little finger; the stick is to be held in the flame of a gas or spirit lamp for a few moments till converted into a live coal, and applied through a glass or wooden speculum. His directions are:—"Take the caustic in the forceps and apply it about four or five lines from the os to the lip which is most hypertrophied. Now, if you make slight pressure for a few seconds you will destroy tissue over a space of about the size of a three-cent. piece, and of about two lines in depth; the pain is very slight. On withdrawing the cautery I sponge the part with cold water. I then introduce a pledget of lint saturated with glycerine, and keep the patient in bed for forty-eight hours." The actual cautery may be applied once a month. I have but recently commenced to try this method; I believe, however, that I shall derive good results from the practice.—*Medical Press and Circular, Dec.* 31, 1873, *p.* 583.

121.—ON PELVIC HÆMATOCELE.

By Dr. Alfred Meadows, Physician-Accoucheur to St. Mary's Hospital, and Physician to the Hospital for Women and Children.

[The term "Pelvic Hæmatocele" is confined to effusions of blood within the peritoneal cavity; when the blood is effused

into the cellular tissue beneath the peritoneum it is called "thrombus." The latter must be extremely rare as Dr. Meadows states that he has never yet met with a case.]

I believe that all cases where the tumour is considerable are cases of true intra-peritoneal hæmatoceles: it is important to bear this in mind, because obviously it has a bearing upon the question of treatment, especially in its surgical aspect; at least I should have less hesitation in tapping a hæmatocele where I knew that the blood was effused and encysted in the peritoneal cavity than I should in puncturing a thrombus with the blood extravasated into the cellular tissue. The risk of mischief resulting is, I believe, far greater in the latter case than in the former. Such, at all events, is my experience in regard to tapping pelvic swellings.

Now, as to the trustworthy symptoms which will in general enable us to diagnose cases of true pelvic hæmatoceles, they are typically seen in the following case, which I have elsewhere recorded:—

Mrs. S., aged thirty, married ten years, and has had two children, the youngest being three years of age, consulted me on November 14th, 1870, for a leucorrhœal discharge, which, she had been told, was due to an ulcer of the cervix uteri. On examination, I found that the uterus was large, measuring three inches in length; the mucous lining of the cervix was everted, and deeply injected, being what is called by some authorities in a state of granular erosion. All the culs-de-sac were entirely free from any deposit whatever. On the day following she went out to dinner, and, although it was very cold and wet, she determined to walk in her light evening dress to the house she was going to visit, which was only a few doors off. Menstruation was about due at the time, but was frequently irregular, so that she could hardly be sure of the date. She had scarcely sat down to dinner when she was seized with severe pain in the hypogastric region, which compelled her to leave the room; and she was carried home, feeling very faint and ill. Some hot brandy-and-water was taken at once, and warm fomentations were applied. The pain, however, continued with but slight abatement, and the next day I was called in. On examining per vaginam, I found a swelling behind the uterus, stretching across from side to side, and pushing that viscus slightly forwards; it also bulged a little into the vagina. There was extreme pain and tenderness in the lump, and also on deep pressure into the pelvis from above. The swelling was somewhat elastic, but more boggy. On the following day the swelling had increased; there was more pain; pulse 120, tongue furred, expression anxious. Nothing could be felt externally. Next day there was still

greater increase and bulging of the swelling per vaginam, which now invaded the left side of the pelvis, pushing the uterus against the right obturator foramen. The pain was not so severe, but differed in that it was of a forcing, bearing-down character. The lump was more distinctly elastic, and I had no doubt that it contained fluid. It had pushed Douglas's pouch so low down as to be quite near the vaginal outlet, and might have been tapped with great facility, either per vaginam or rectum. On the fourth day there was slight aggravation of all the symptoms, with a relative increase in the size of the swelling, which, however, could only be felt externally on pressing into the pelvis. It did not rise above the level of the pelvic brim. There was no obstruction either of the bladder or rectum, though the bowels acted with difficulty and great pain, and only after aperient medicine. During the course of the next ten days the symptoms greatly abated, the pain diminished, and the swelling grew less and less, so that on December 14th, just a month after the commencement of the attack, it was noted that "there is hardly a trace of the lump left; but a kind of ridge may be felt behind the uterus, forming, as it were, a cast of the peritoneal fold in that direction." Even that ultimately disappeared, and when I examined her early in February nothing abnormal was discoverable.

On the 15th of that month, it being a menstrual period, she again exposed herself to cold, and was seized in almost precisely the same manner. I saw her the day after, and on examination I found that the uterus was depressed, pushed somewhat forwards and to the left side by a boggy, putty-like mass, which occupied the posterior and part of the right lateral cul-de-sac. Defecation was difficult and extremely painful. There was very slight sanguineous discharge from the vagina, and violent bearing-down pain, as if everything would force from her. The swelling at no time appeared to be larger than a good-sized orange. A few days subsequent to this, and after a good deal of forcing pain, she expelled a small mass, which, on examination, proved to be nothing more than a decolourised fibrinous clot, entangling a good deal of uterine epithelium. After this she felt great relief; the catamenial discharge came on much more freely; the post-uterine lump began to diminish in size; and she made a rapid and complete recovery, so that when I saw her a few months afterwards the uterus was quite movable; no deposit could be felt in either cul-de-sac, and nothing abnormal was discoverable.

Now it seldom falls to our lot to be able to demonstrate, as was done in this case, the rapid formation of a post-uterine swelling coincidently with the occurrence of symptoms of acute

pelvic inflammation. It so happened that a few hours before the attack came on I had the opportunity of examining this lady, and was satisfied that no pelvic swelling existed. A similar examination, made a few hours after the attack set in, showed the existence of a soft, elastic tumour behind the uterus, filling up Douglas's pouch, and pushing the uterus forwards against the pubis. Now there is no swelling or tumour that I know of in connexion with the female generative organs which can be formed so rapidly as this, except pelvic hæmatocele. This fact alone, therefore, was sufficient to warrant the diagnosis, and it was corroborated entirely by the accompanying symptoms. We cannot, however, reckon upon such opportunities as were afforded me in this case; and, in the absence of that knowledge, our diagnosis will be founded partly upon the history of the case and partly upon the character of the pelvic swelling.

As regards history, I find that in all the cases which have come under my care there has been a more or less distinct history of uterine or ovarian trouble, which has, as it were, led up to and culminated in the attack which now claims attention. This is more particularly observed in those cases where the hæmatocele is not directly and immediately connected with menstruation. In the majority of cases there is this connexion, and the hæmatocele results either from the regurgitation of the menstrual fluid from the uterus along the Fallopian tube into the peritoneal cavity, or else the afflux of blood which usually takes place to the generative organs at the time of menstruation leads to such an engorgement of the venous plexuses that rupture occurs at some point where either a varicose or other diseased condition existed, which predisposed to the catastrophe. In a certain number of cases—but they are, in my experience, very rare—the hemorrhage occurs in connexion with pregnancy, or rather with delivery, either at term or, more commonly, prematurely, and especially during the earlier months. Probably in these cases the diseased condition, whatever it be, which gives rise to the hæmatocele after the abortion is the principal agent in bringing about the premature expulsion of the ovum. And here also careful inquiry will serve to elucidate the fact insisted upon—viz., the existence of symptoms of uterine or ovarian disease prior to the occurrence of pelvic hæmatocele. Indeed, I do not think it possible for such an accident, if I may so call it, to occur in a woman perfectly healthy in those parts. Happily for the patient though unfortunately for science, these cases seldom end fatally, and our opportunities, therefore, of examining the parts and of demonstrating the origin of the mischief are few and far between. I believe that a varicose condition of the

veins of the pampiniform plexus, resembling varicocele in the male, is of very common occurrence, and may have much to do with the disease we are considering. All this, of course, has reference only to antecedent history and to predisposing causes.

The symptoms which characterise the attack itself are generally very well marked, and although in themselves it would, perhaps, be difficult in some cases to distinguish them absolutely from those which occur in certain acute pelvic inflammations, such as cellulitis or peritonitis, yet in general there are important differences which, taken in connexion with a digital vaginal examination of the resulting pelvic swelling, are sufficiently characteristic to make the diagnosis a matter of tolerable certainty.

Probably the only conditions with which pelvic hæmatocele is likely to be confounded are pelvic cellulitis and pelvic peritonitis; but in regard to each of these there are some few points of dissimilarity in the character of the symptoms, and there are still greater differences in the local physical signs, especially in regard to the formation of the pelvic swelling, its situation, consistence, and general characters.

In all three, when fully developed, there are the usual signs of inflammatory action—namely, local pain and general febrile disturbance. To some extent they resemble one another in the mode of attack, but though each may begin rather suddenly, the symptoms of cellulitis and peritonitis are seldom so severe at first as hæmatocele; the pain of cellulitis is not nearly so sharp as that of the other two, and hæmatocele is generally the most severe of all. Faintness, great prostration, and even collapse to an alarming extent, frequently accompany the accession of hæmatocele; neither of these occurs in connexion with cellulitis or peritonitis. On the whole, it may be said that, taking average cases of these three affections, pelvic hæmatocele is characterised by symptoms which are more sudden, severe, and alarming than either of the other two—it, in fact, shows its traumatic character very distinctly; and pain, prostration or collapse, with symptoms of internal hemorrhage, are its leading features. The pain is of course very local and limited, and it has a peculiar forcing, bearing-down character, a feeling which is described sometimes as if everything were being forced away.

With the occurrence of such symptoms, a vaginal examination will in general suffice to make the diagnosis clear. If circumstances have enabled us, as in the case already detailed, to demonstrate the sudden formation of a pelvic swelling, situate chiefly in the post-uterine region, that may be taken as conclusive evidence of hæmatocele; for not only does the swelling

in cellulitis form much more slowly, but it differs also in situation and character. It is at first neither in front of nor behind the uterus, but on one side, originating as it does in the cellular tissue of one or other broad ligament. In pelvic peritonitis the swelling forms even still more slowly, is generally behind the uterus—not on one side,—and more limited in extent than either of the other two.

Again, there are differences in the character of the swelling. In hæmatocele it is at first tense, elastic, fluid, and fluctuating; as time goes on, it becomes boggy, doughy, non-fluctuating; then firmer and firmer, till it is almost hard, and with this change it contracts considerably, getting smaller and smaller, till it finally disappears. Cellulitis, on the contrary, is small at first, and gradually increases in size, becoming softer and more elastic as it increases. It sometimes changes its brawny character for one in which fluctuation can be distinctly made out. In all these respects it resembles pelvic peritonitis, and so far both differ from pelvic hæmatocele; but they in their turn differ from one another in this respect, that in cellulitis the swelling is one-sided, while in peritonitis it is situate either anteriorly or posteriorly.

Lastly, in cellulitis the uterus is usually displaced latterly, being pushed to one side by the swelling formed in the broad ligament of the opposite side; while in hæmatocele and in pelvic peritonitis the uterus is displaced forwards or backwards, but most commonly forwards, and much more so in hæmatocele than in peritonitis.

Such are the characteristics of the pelvic swelling in these three conditions. Their differences are in general sufficiently marked to enable us to diagnose them with tolerable certainty, especially when taken in connexion with the history and symptoms past and present. Neither the history nor the physical signs taken separately are in themselves sufficient to establish the diagnosis. Both must be taken together; but a minute and careful examination is of the utmost importance, not only for the purpose of diagnosis, but still more in reference to treatment, and especially in regard to that mode of treatment which I believe to be the best and most successful. I have a strong impression that if the symptoms and physical signs of pelvic hæmatocele were more generally understood and appreciated, we should find that cases of this affection are more common than is generally supposed, and that it is not such a medical curiosity as some imagine. Cases of pelvic cellulitis, pelvic peritonitis, and pelvic hæmatocele have no doubt just sufficient in common to make an error of diagnosis pardonable; but I would venture to hope that mistakes of this kind may be less common in the future as the disease becomes better understood.—*Lancet, Nov.* 15, 1873, *p.* 698.

122.—NOTE ON SPONGE TENTS.

By LAWSON TAIT, Esq., Birmingham.

The extremely offensive smell of a sponge tent removed from the uterus after having been there only a few hours must be familiar to all who have used one; and, considering the frequent use of sponge tents, it is singular that this abominable putridity does not more frequently produce serious mischief than it would seem to do. Although I have used a very large number of these tents, I have never seen them do any harm until recently, when a case of fatal septic peritonitis occurred in my hospital practice which I could trace only to the septic influence of a sponge tent.

Sponge tents are far safer than those made of sea-tangle, and it has long been one of my objects to remove the above-mentioned objection to their use. I have tried charging them with various disinfectants, but without any result, until last week I made an experiment with oil of cloves, and I have found that a tent charged with 5 per cent. solution of oil of cloves will remain in the uterus for twenty-four hours without becoming offensive in the slightest degree. There can be no doubt that such a tent is far safer than those ordinarily in use, and it is certainly much more agreeable to the operator. Messrs. Krohne and Sesemann, of Duke-street, W., make these tents according to my formula.—*Medical Times and Gazette, Jan.* 10, 1874, *p.* 35.

ADDENDA.

123.—ON THE ACTION AND USES OF CROTON-CHLORAL HYDRATE.

By Dr. Oscar Liebreich, Professor of Materia Medica in the University of Berlin.

[Croton-chloral is a new remedy, allied to chloral-hydrate in its action, but differing in some most important points from it.]

When chlorine gas acts on aldehyde, croton-chloral is formed, as has been demonstrated by Dr. Krämer and Dr. Tinner. In order to avoid a mistake which is apt to be caused by the name, I must here remark that this body possesses no relation whatever to croton-oil, although its chemical constitution proves it to be the chlorated aldehyde of crotonic acid. Croton-chloral differs in its outward appearance from hydrate of chloral, by its being dissolved with difficulty in water, and by its crystallising small glittering tablets. Its action, though similar to that of hydrate of chloral, differs widely from the latter with regard to its physiological effects. Four *grammes*, or a drachm, of this substance, dissolved in water, and introduced into the stomach, produce in the course of from fifteen to twenty minutes a deep sleep, accompanied by anæsthesis of the head. Whilst the eyeball has lost its irritability, and the nervus trigeminus shows no reaction whatever on being irritated, the tone of the muscles remains unaltered.

I have experimented with this remedy on maniacs during an attack of mania. They remained quietly sitting on their chairs in a deep sleep, their pulse and respiration being unchanged, for two whole hours together. If anæsthesis had reached so high a degree in consequence of the application of hydrate of chloral, the patients would have dropped from their chairs, and both their pulse and respiration would have been considerably retarded. I have seen croton-chloral acting in the same way on healthy individuals. In some cases of tic douloureux, the remarkable phenomenon is exhibited that pain ceases before sleep sets in. I am sorry to say, however, that this remedy acts only as a palliative in this dreadful disease. I neverthe-

less prefer its action to that of morphia, because it has effects as good as the latter remedy, without being so detrimental to the constitution in general. I have never observed any unfavourable effects of croton-chloral on the stomach or any other organ, although I have made frequent experiments with it.

The indications for the use of this remedy are to be found—1. In cases where hydrate of chloral is inapplicable on account of heart-disease; 2. In cases of neuralgia in the district of the nervus trigeminus; 3. In cases where very large doses of chloral are necessary to produce sleep. I there recommend the addition of croton-chloral to hydrate of chloral.

Whilst examining the difference between the action of hydrate of chloral and that of croton-chloral, I have discovered the remarkable fact that it is not the first, but the second, product of decomposition of the latter substance which is brought into action, on account of the first being too rapidly destroyed. Croton-chloral, when subjected to the influence of an alkali, first forms allyl-chloroform, a trichlorated body, which is rapidly decomposed into a bichlorated substance called bichlorallylene. Now, both chloroform and trichlorated substances act, as I have shown, in their first stage on the brain, in the second on the spinal cord, and in the third on the heart. The retardation of respiration is to be explained by the agency of these substances on the last mentioned organ. Bichlorated substances act differently, as is proved by bichloride of ethylene. Even if the circulation of the blood in an animal have been stopped by this latter agent for one minute, life may be restored by artificial respiration, which is impossible whenever trichlorated substances have produced this effect, in which case the muscles of the heart remain paralysed. Well, in animals poisoned by croton-chloral to such a degree that both circulation and respiration are stopped entirely, artificial respiration is able to restore the action of the heart immediately, and the life of the animal may thus be saved. Bichlorallylene, inhaled by the lungs, produces the same effect on animals as croton-chloral. We thus see these bichlorated substances acting on the brain, spinal cord, and medulla oblongata, but not on the heart, which explains the fact that both respiration and circulation remain unaltered in man by a medicinal dose. It is a highly interesting fact, however, that under favourable conditions, we still are able to produce in animals the effects of the first product of decomposition of croton-chloral—*i.e.*, of the trichlorated substance or of allyl-chloroform. In order to observe these effects, it is necessary to introduce immense doses of croton-chloral into the body, when paralysis of the heart actually does ensue.—*Brit. Med. Jour.*, *Dec.* 20, 1873, *p.* 713.

124.—COCA: ITS THERAPEUTIC USE.

By Dr. E. H. Sieveking.

Many years ago my attention was drawn to the reputed restorative powers of "coca," by a pamphlet written by M. Mantegazza of Milan, of which I gave a short *résumé* in the British and Foreign Medico-Chirurgical Review for July, 1859. Although I have found it alluded to in works that dealt with the Indians of South America, I have never had the good fortune to meet with any medical man who had a personal acquaintance with it, nor till now have I succeeded in obtaining the substance itself. Through the kindness of Mr. Fernandos, a chest of coca leaves, as they are used by the Indians of Bolivia, has reached me, and I have placed it with Mr. Squire, her Majesty's chemist, from whom samples may be obtained. The leaves are derived from the *Erythroxylon coca*, a shrub that is indigenous in South America. They are chewed by the Indians, who are stated by Mantegazza, Moreno, Guibourt, and others, to go through laborious work for several days and nights in succession, without exhaustion, although they have little or no food, provided that they have a supply of about eight ounces of coca-leaves to chew during the day. An infusion of half a drachm to a cup of hot water is said to produce beneficial effects.—*British Medical Journal*, *Feb.* 21, 1874, *p.* 234.

125.—TOOTH-EDGED CUTTING SCISSORS.

By Dr. Benjamin W. Richardson, F.R.S.

I have recently had constructed for my use a pair of tooth-edged cutting scissors on the plan shown in the diagram. The scissors are of the ordinary construction in all respects except in the cutting edge. The cutting edge of each blade, instead of being even and sharp, is divided into finely-pointed teeth, each tooth being directed with a slight inclination towards the handle of the scissors. When the blades meet the teeth cross each other, and as they pierce any structure that may lie between them, they crush also, between their surfaces.

If a piece of moderately firm substance be placed between the blades—a piece of paper or of thin card, for example—the scissors perforate it in a series of perforations resembling what is seen in the postage stamp—that is to say, they do not cut

clean through the substance so as to leave it in two distinct parts at once. A little lateral or half-rotating movement of the closed blades is, however, sufficient to tear through the still connected lines of substance and to complete the separation. The same occurs if the substance placed between the blades be a portion of soft animal structure, only that more force is required in the lateral or rotating movement to cause complete separation. The parts punctured are crushed between the teeth, and are separated by the twist or torsion.

I find these scissors useful in dividing directly and quickly structures in which there are many minute bloodvessels, and which, when divided by the knife, bleed freely. These toothed scissors, as they can be made at one and the same time to pierce, crush, and twist, control bleeding remarkably.

It occurred to me at first to modify a pair of torsion forceps so as to make them cut with toothed edges at the part where they compress, and I used a forceps thus modified with success. Mr. Perkins, jun., of Baker-street, brought me one day a patient with an epulis. The gum beneath the base of the tumour was much thickened and vascular, but with my toothed forceps I cut through easily, and removed the whole mass quickly and thoroughly, effecting a good cure. But I found a want of purchase in handling the forceps, so I determined to carry out the method with scissors as above described. Mr. Perkins, who, as a dentist, is as mechanically skilful as he is energetic, earnest, and intelligent in his professional work, was good enough to make a model of my design out of a pair of ordinary surgical scissors, and from this model Messrs. Krohne and Sesemann, with their usual facility, have constructed the special instrument figured in the diagram.

I have put the scissors to a good test in a case of epulis, in a patient under the care of my friend Dr. Davson. In this instance the tumour was increasing rapidly, and three teeth were involved in it. It was very vascular, had a broad base, and might at first sight have been taken for a malignant rather than for a fibrous tumour. The three teeth being extracted, I found I could get a deep grasp of the tumour between the blades of the scissors. I carried the teeth of the scissors well through the base of the tumour, crushing some portion of bone in the way, and gently and easily twisted the mass off, and lifted it away upon the blades without the loss of any blood whatever. The healing in this case was rapid and good.

These tooth-edged scissors may, I think, be usefully modified. Made with longer handles and a little curved in the blades, they could be used for removing abnormal growths from the surfaces of internal organs—such as the uterus—by the com-

bined processes of pierce, crush, and twist. Made with very delicately formed teeth, and altogether small, they might be used for removal of abnormal growths from the ear, the nostrils, or the eyelids. They could easily be constructed for the removal of the tonsils, and they are ready for use in cases of piles. They will probably turn out serviceable in operations for removal of portions of the tongue.

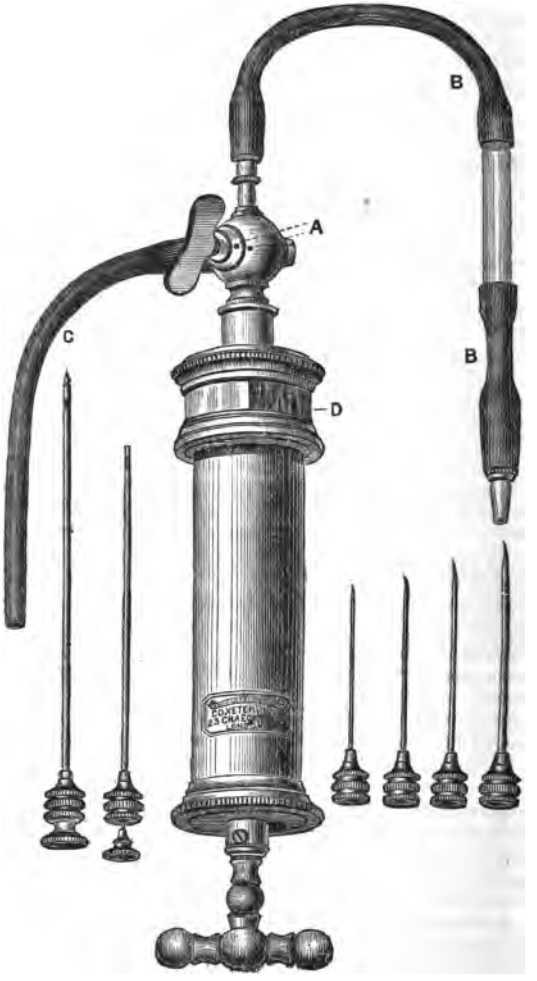
[Mr. C. F. MAUNDER, Surgeon to the London Hospital, one day accidentally saw Dr. Richardson's new invention, the "Tooth-edged Cutting Scissors," and determined to try the instrument upon a case of internal hemorrhoids upon which he had arranged to operate in the afternoon.]

The case was a typical and uncomplicated one of internal piles, which I proposed to ligature. Having seized one hemorrhoid, I proceeded to sever with the scissors the mucous membrane at its junction with the skin at the base of the pile, in order to form a groove for the ligature to lie in. This instrument effected my object most readily, and *without bleeding*, the structure divided requiring perhaps two applications of the toothed scissors where one of the common scissors suffices. My colleague Mr. McCarthy, and Mr. R. Kershaw, late House-Surgeon of the London Hospital, who assisted me, noted the absence of bleeding. Five hemorrhoidal tumours were thus treated.—*Med. Times and Gazette, Dec.* 20 *and* 27, 1873, *pp.* 689, 714.

126.—COXETER AND SON'S ASPIRATOR.

Various modifications of the aspirator have been brought out by different instrument-makers; and Messrs. Coxeter's is one of the latest, if not the best. The objection to the glass cylinder adopted in many instruments is the difficulty, if not impossibility, of making it so true that the piston shall fit accurately throughout and maintain a perfect vacuum. In this new instrument Messrs. Coxeter have reverted to the brass cylinder, but have fitted to its lower end a narrow glass ring, through which the character of the fluid withdrawn can be readily seen. The aspirator is fitted with an ingenious "three-way" stop-cock, by which the use of a second stop-cock, which is always troublesome in use, is dispensed with. As it is found impossible to make the ordinary flute-key stop-cock of the stomach-pump sufficiently air-tight for purposes of aspiration, this arrangement seems to be the most satisfactory which can be obtained. In addition to the glass in the cylinder, the tube to which the aspirating needle is attached is fitted with a piece of glass tubing, so that the nature of even a very small quantity of

fluid may be at once ascertained. The needles themselves are made of seamless steel, and are, therefore, more durable, and



less liable to become impermeable from rust.—*Lancet, Feb.* 14, 1874, *p.* 234.

127.—TREATMENT OF BRONCHOCELE BY PHOSPHORUS.

By S. Messenger Bradley, Esq., Manchester.

I should like to mention a disease which appears to be influenced in the most beneficial and rapid manner by the administration of phosphorus—I mean bronchocele. I have only notes of one case, but this is an instructive one. The patient, a girl aged 20, had an increasing goître for some months, when she was placed on the usual iodine treatment, with no benefit whatever. After a month's interval of no treatment, phosphorus was given in doses ranging from the one-fiftieth to the one-twentieth of a grain, with the effect of directly causing a diminution in the size of the gland; the decrease went on steadily until the neck, in a month, measured two inches less than it did before the commencement of the treatment.—*British Medical Journal, Nov.* 29, 1873, *p.* 630.

128.—ON ONYCHIA MALIGNA.

By William MacCormac, Esq., Surgeon to and Lecturer on Surgery at St. Thomas's Hospital.

[This disease, as is well known, is of the most inveterate nature, the pain being most intolerable—all local treatment, and even the evulsion of the nail, generally producing but temporary good effects. The only treatment which is effectual is the complete excision of the secreting structures at the root of the nail. This is a severe operation.]

The appearance of the ulcer is something quite peculiar. It has an ashy-coloured surface, with shreds of tissue attached to it; often red points dot it here and there, and the discharge has an intolerable smell. The edges, owing to the inflammatory swelling, are much elevated, and are often as sharply defined as possible. The pain is usually continuous, and very severe, of a gnawing character, aggravated by warmth, and preventing sleep in many cases.

The malady, if not interfered with, continues for years, there being little or no tendency to spontaneous cure. There is a great tendency to relapse after simple evulsion of the nail, while complete excision of the matrix ensures a permanent cure.

It is the opinion of some eminent surgeons, that the disease is often the extension of hereditary syphilis; but I could never satisfy myself that there was any such connexion. I believe it, at all events, to be rare. In the many cases I observed, the affection seemed always clearly traceable to local conditions. Often it would arise in persons of otherwise unimpaired health; but very frequently it was observed amongst the indifferently fed overworked factory hands.

From what I have been able to learn, it is in most places a rare disease. Mr. T. Smith, in vol. v. of Holmes's Surgery, states that it is almost confined to children under ten years of age, and is usually observed on the fingers. Amongst seven thousand surgical out-patients under twelve years, he noticed only nine cases, and these occurred between the ages of one year and seven.

My attention was recently recalled to the subject of onychia by reading an elaborate monograph on the disease by Professor Vanzetti, of the University of Padua. After giving many most interesting historical and other details, he recites the particulars of twelve cases of onychia maligna treated by the application of powdered nitrate of lead to the surface of the ulcer. The result seems to have been simply marvellous. In each case, a speedy and permanent cure rapidly followed, with the reproduction of a perfectly healthy nail. Dr. Vanzetti's antecedent experience had been somewhat similar to my own; he had exhausted, without satisfaction, all sorts of remedies, and relied on excision for a cure—a means which, on account of its severity, he very reluctantly employed.

Very shortly after the first application, all pain ceases; the swelling soon begins to diminish; also the suppuration, which loses its fœtor; and it is not rare, at the end of five or six days to see a perfectly healthy healing sore.

Professor Vanzetti was much impressed with these statements, and determined to give the remedy a trial at the earliest opportunity. After a time, a boy of seven presented himself in his *clinique* with onychia of the middle finger of one year's duration. The pain was so excessive as to cause a species of convulsion every night. No form of treatment had produced the slightest benefit. The last phalanx was double its natural size, livid red in colour, ulcerated along the sides and upper edge of the nail. The loose portion of nail was removed by scissors, so that the powder might be freely applied to every part of the diseased surface. Considerable pain was complained of until evening, but then the child slept soundly all night. On removing the hard crust which formed over it, the ulcer was seen, two days afterwards, to be looking well. Three applications in all were made, and in eleven days the sore completely healed up. A year afterwards the little patient was seen by Dr. Vanzetti, who found that a new and perfectly well formed healthy nail had grown, only differing from that on the opposite hand in being a trifle larger.—*British Medical Journal, Dec.* 6, 1873, *p.* 653.

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